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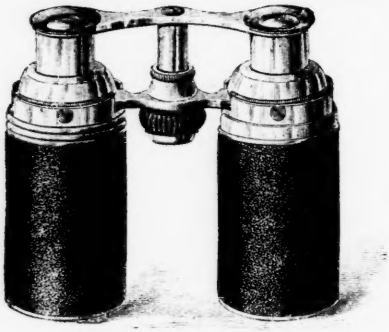

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THE MONTHLY

JOURNAL OF SCIENCE.

JULY, 1879.

I. THE ANOMALOUS SEASON.

THE exceptional weather which the British Islands, along with Western Europe as a whole, have experienced during the last eight months, makes meteorologists of us all. Men are eagerly asking whether the present season is a calamity hitherto without precedent, showing perhaps a lasting deterioration of our climate, and announcing the return of a glacial epoch, or a mere periodical phenomenon recurring at fixed intervals? If we accept the latter, or cyclical, hypothesis, of how many years must we suppose the cycle to consist? There is a widespread tendency to connect recurrent phenomena, famines, pestilences, inundations, storms, and even commercial depressions, with the periodicity of the sun's spots—a term between ten and eleven years. That many facts speak in favour of such a supposition is evident, and it may therefore be worth our while to consider whether a pre-eminently severe and ungenial season may be looked for every ten or eleven years. We must ask, in the first place, how, in looking back over the past, are we to recognise these bad seasons? Mere meteorological returns, average, maximum and minimum temperatures, quantity of rainfall, and even hours of sunshine and proportions of the equatorial and the polar currents, throw but an imperfect light on the subject. It is not so much the quantity of cold, of wind, of rain, and of clouded skies, as their manner of distribution, which decides whether a given season shall be pleasant and fruitful or the reverse. The best criterion of the character of the winter and the spring must be found, we think, in certain pheno-

mena of the organic world, such as the leafing and flowering of trees. In like manner the nature of the summer and the autumn may be most conveniently judged by the ripening of seeds and fruits, by their abundance or scarcity, and their quality. This is merely extending a principle fully recognised in climatology. If we say that in a certain district the sugar-cane, or the banana, or the cacao, or the date-tree flourishes, everyone versed in botanical geology will at once form a fairly precise notion of its climate. In like manner if we say that in England, in a certain year, the sloe was in full bloom on May 13th, we at once characterise that season.

What evidence, therefore, has the vegetable life of the country to give for or against a recurrence of bad seasons every ten or eleven years, between each pair of which a strikingly good season is interposed, though not generally in the midway?

Within the past forty years there have been four, or perhaps five, signally inclement years: the winter 1844-45, with the succeeding spring and summer; 1854-55; 1860-61; 1870-71 (a less decided case); and lastly, 1878-79. The first of these seasons showed the ordinary features of a prolonged winter, a backward spring, and a wet and cold summer. In some parts of the north of England skating was practicable in March. The Rev. L. Jenyns—who has tabulated the earliest, average, and latest times of a number of organic phenomena, as observed in the years from 1820 to 1831—notes that in 1845 these occurrences were later than his latest dates, the differences being, however, variable in their amount. Hence we learn that different plants, as might be expected, are very unequally affected by bad weather in their times of leafing or flowering. In a bad season there is not merely a general translation of periodic phenomena to a later part of the season, but there is also respective displacement. Animals, we must observe, are a less trustworthy guide than plants, because they more readily escape our notice. We may, for instance, register the first appearance of some bee or butterfly on a certain day, whereas had we been in another field we might have observed it a week earlier. The summer of 1845 fully maintained the character of the earlier portion of the year.

The season 1854-55 was in almost every respect similar. The cold set in early and continued late. Frosts, snowstorms, and sleet continued into June, and on the morning of July 1st a frost cut off the potato-plants over much of the north and east of England. No fine weather ever came,

and the harvest was late and deficient in quantity and quality.

In 1860-61 all these characters were still further intensified. Already in October, 1860, so severe a frost occurred that rocks and railway embankments were fringed with icicles. On the morning of Christmas-day was observed the severest cold probably ever experienced in England. Large well-established apple-trees, and even oaks of a yard in girth, were killed. Through the spring and summer the polar current still retained its ascendancy, and in many parts of the north of England frost was experienced almost every night in the month of July. The corn rotted where it had grown, and was ploughed into the ground in the ensuing season.

The claims of the season 1870-71 to rank in this evil catalogue are not unquestionable. It was undoubtedly the severest winter between 1860-61 and 1878-79, and the snow which fell on December 22nd lay unmelted into February. There was, further, no summer weather till the beginning of August. But November and the earlier part of December, 1870, were mild and genial; and in March, April, and May, notwithstanding an excess of easterly winds, the leaves and flowers were not far behind the average in their time of appearance.

We now come to the present season, the characteristics of which are too fresh in the public mind to need recapitulation. The following facts will show the effects upon vegetation :—

	Mean Time.	1879.
Sloe and plum flower ...	April 7	May 13
Maple in leaf	April 10	May 24
Lilac flowers	April 30	June 3
Ash in leaf	April 30	May 25
Oak	May 2	May 30
Horse-chesnut flowers ...	May 3	June 12
Whitethorn flowers... ...	May 5	June 4 (a few flowers in sheltered places).

The animal world is much less gravely affected. The cuckoo was first heard and seen here on April 25th, two days before its mean time of appearance, and the swallows returned to us on April 20th, only one day behind its average. The nightingale, also, was singing at its accustomed season, though among branches nearly leafless. These facts, we

submit, show that migratory birds do not hasten or delay the time of leaving their winter-station by reason of any mysterious knowledge of the weather prevailing in the country whither they are bound. Among insects *Gonopteryx Rhamni*, *Vanessa Urticæ*, and *V. Io* were seen as early as March 7th, their respective average times, according to Jenyns, being March 10th, 27th, and 31st. *Euchloe cardamines* appeared this year on June 5th—nearly a month behind its average time. *Callidium violaceum* was seen this year exactly at the same time as in the two previous seasons. Aphides have already descended in swarms upon fruit-trees, and are committing serious damages. Hence we may argue that long and severe winters have little direct influence upon insect life, and that there is consequently little hope of frost ridding us of noxious species. Not a few countries whose winter temperature is far below our own are much richer both in insect species and individuals than in England.

If we next look to the intervals between the five exceptionally bad seasons we have mentioned, we find them separated respectively by 10, 6, 10, and 8 years. If we reject the claims of 1870-71 we have, of course, instead of the two last mentioned, a single space of 18 years. Before attempting any comment upon these figures it may be useful to turn to the exceptionally fine seasons. As a starting-point we may take 1868 as utterly unquestionable in its character. It was preceded by a winter milder and shorter than the average. The equatorial current early got and maintained the upper hand. March was milder than an ordinary May, and the middle of July brought a splendid wheat-harvest. In October wet weather set in, but unattended with cold. Christmas-day was warm and genial, with primroses and violets in bloom. The winter was not marked by either severe, lasting, or often-repeated cold. The two following summers and the intervening winter, though inferior to the splendid season of 1868, were, to speak in the most guarded manner, a very fair average. Since then no summer of an exceptionally fine character has occurred, so that we have the whole period between 1870-71 and 1878-79 without an intervening season of heat.

Turning to the past, if we wish to find another year similar to 1868 we must go back forty-three years, to 1825, when the wheat-harvest also fell in July. Still there have been in the meantime seasons very much superior to anything experienced since 1869. Thus the summer of 1857

was dry and warm. In the succeeding winter the only frost experienced was in February, and was of no long duration. The summer of 1858 was even finer than its predecessor, and was followed by winter free from frost and snow, save from about November 12th to 17th. Here, therefore, we have two epochs of heat separated by an interval of about ten years. Had the series been carried out we should have had another warm epoch in the year 1878 or 1879, or in both, followed by unusual cold in 1880-81.

It will be apparent, on examining the above facts, that an unusually warm summer does not stand alone. As a rule it is preceded or followed by mild winters, and may be regarded as a culminating point from which the temperature descends on either side through a total space of about two years. With exceptionally cold and prolonged winters the case is inversely similar; they are either ushered in or succeeded by chilly, wet summers, and the depression of temperature is rarely limited to a single season. Thus the summer of 1862, though not absolutely frosty like its predecessor, was still cool, and not until 1863 had we any true summer weather.

Much of this becomes intelligible if we remember that the character of the seasons in Western Europe is more immediately at least determined by the conflict of the two great atmospheric currents, the polar and the equatorial. In what may be called average years the opponents are about equal in strength, and have their battle-ground over the British Islands. Hence the proverbial fickleness of our climate, the cloudiness of our heavens, and constant possibility of warm weather at Christmas, or of frost in summer or early autumn, according as one or other of these aërial rivals wins a temporary advantage. Sometimes, however, though rarely, the equatorial current gets decidedly the upper hand, as in 1868, and retains it for a year or more. In this case the battle-ground is transferred to Norway, whilst we are deeply and continually immersed in southerly winds. The weather then is not merely warm, but dry, for these winds do not deposit their moisture unless chilled by the near proximity of a polar current of air. More commonly, however, as in the present season and in 1861, the polar current gets and maintains a long superiority. The conflict is then transferred to Spain, and we are completely plunged in icy winds. If a southerly or south-westerly air blows for a short time, its stock of moisture is immediately precipitated by contact with the chilled earth or by polar

currents in the upper regions of the air, so that the only result is a torrent of rain followed by a fresh rush of the north-east wind. Even the storms which cross over from America fail to give any relief; they are deflected southwards by some unknown cause, and strike the coasts of France, Spain, and Morocco.

But these considerations, after all, take us but a very little way; we ask why should the equatorial current prevail in one season and the polar in another? Why is either, when supreme, apt to retain its sovereignty so long? Why is the change from a cold to a warm epoch, or inversely, generally effected about the autumnal equinox? Although the cold and the warm seasons by no means exactly and invariably succeed each other at intervals of from ten to eleven years,—or otherwise we should now be enjoying a recurrence of 1868,—we feel by no means free to maintain that the alternation in question is not connected with the periodicity of the solar spots. The exceptions may very possibly be residual phenomena due to the intervention of some as yet unsuspected cause, which may either displace the epochs of greatest heat and cold or may obliterate one of them altogether.

A practical consideration is how the approach of an abnormal season may be foreseen. If northerly winds begin in October, and continue with little intermission till the end of the year, there is already room for grave apprehension. But if, after an early commencement of winter, there is no decided change before the end of February, a cold late spring and a chilly summer become almost a matter of certainty. Hence it will be seen that the writer is not one of those who expect heat and drought as characteristics of the approaching summer.

We may at any rate safely say that a severe and prolonged winter is far from announcing a warm summer. Nor is it safe to conclude that in a late spring the fruit will escape the effects of frosty nights. Such frosts in a late season simply occur in June instead of May.

II. THE HISTORY OF VESUVIUS DURING THE LAST TEN YEARS.

By G. F. Rodwell, F.R.A.S., F.C.S.,
Science Master in Marlborough College.

THE earlier eruptions of Vesuvius were of a violently paroxysmal character. The pent-up forces did not discharge themselves little by little at frequent intervals of time, but a long period of tranquillity, or of slight dynamic activity, was followed by one of great eruption. Prior to the eruption of A.D. 79 the mountain was not even regarded as an active volcano. From this period until the year 1139 there was no eruption of any magnitude, and then more than five hundred years elapsed before the mighty outburst of 1631, which entirely changed both the shape of the mountain and the aspect of the surrounding country. From this time the eruptions occur more frequently. During the 18th century the mountain was rarely in a state of rest, and more than four-fifths of the recorded eruptions occurred in this and the present century. These, as far as, and including the eruption of 1868, have been described so fully by Prof. Phillips in his admirable account of the mountain, that we need only refer to them, and may pass on at once to the history of Vesuvius since 1868.

The eruption of 1868 terminated in November, and the mountain remained quiet till the end of December, 1870, with the exception of a few fumeroles of the crater, which emitted gases, and deposited near their mouths sublimations of chloride and sulphide of copper, and sulphide of potassium. Early in 1871 the seismograph of the Observatory showed signs of disturbance; slight detonations occurred within the crater, and incandescent particles were discharged in small quantity. On January 13th an aperture made its appearance on the northern side of the cone, from which a little lava issued, and around which a small cone was soon formed. A red-coloured smoke soon afterwards issued from it, and incandescent scoriæ were ejected, while the detonations proceeding from the great crater were louder and more frequent. Lava continued to flow from the small cone, at short intervals, until the beginning of March, when the cone subsided, leaving a cavity about 30 feet in depth, the walls of which were covered with sublimations, while at

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the bottom a small cone 6 feet in diameter made its appearance, and ejected smoke and steam with a hissing noise. This lilliputian cone increased in size by the accumulation upon its sides of the scoriæ which it ejected, until it rose to a height of 16 feet above the brim of the cavity; it then commenced to discharge lava, which flowed into the Fossa della Vetrana to a distance of about 1000 feet from the new cone. This lava is very leucitic in appearance, and appears to consist almost entirely, according to Palmieri, of minute crystals of leucite imbedded in a homogeneous paste. The stream continued to flow slowly in the direction of Canteroni for several months.

In October, 1871, another small crater opened near the edge of the large cone of Vesuvius, and in November, lava was emitted from the great crater, which ran down the western slopes of the cone. In the following January the crater of October became active; loud bellowings were heard, and it emitted lava and projectiles. In February the activity somewhat abated, but in March an opening was made in the side of the cone, from which a good deal of lava was discharged into the Atrio del Cavallo. The stream flowed for a week, and then ceased. On April 23rd the seismograph showed a good deal of disturbance, and on the 24th a considerable quantity of lava was emitted, and it continued to flow for two days. Early on the morning of April 26th a number of persons went with inexperienced guides to see the new lava streams: as they were proceeding towards the summit the great cone was suddenly rent open in the north-westerly direction, commencing at the new cone and extending to the Atrio del Cavallo, and a copious torrent of lava proceeded from the fissure. Soon afterwards two small craters appeared near the summit of the principal cone, which discharged incandescent particles and quantities of white ash. The whole series of effects took place so suddenly that eight medical students who had approached too near to the scene of action were overwhelmed by the lava, and a number of persons were injured by the hot ashes and scoriæ. The great fissure of the principal cone was both broad and deep, and it extended for about 1000 feet into the Atrio. A ridge of little hills, like a miniature chain of mountains, marked the course of the fissure. The highest point of the ridge was about 150 feet above the plain. A smaller fissure opened on the south side of the cone, and a small stream of lava flowed from it towards Camaldoli.

The lava which flowed from the great fissure on the north-

west side of the mountain divided itself into two branches, the larger of which was 875 yards in width, and it flowed to a distance of 1425 yards in the Fossa della Vetrana in three hours. It then descended into the Fossa di Faraone, where it again divided into two streams, one of which ran over the lava of 1868 in the Piano delle Novelle, while the other flowed through the Fossa di Faraglione over the lava of 1855, and reached the villages of Massa and S. Sebastiano. The stream passed between them, and afterwards continued its course through an artificially constructed channel, until it reached some highly cultivated ground, which it overwhelmed. The lava moved with great rapidity; between 10 and 11 a.m. it traversed more than 3 miles, and covered an area of 2 square miles. The supply of lava fortunately ceased at this source, otherwise in less than twenty-four hours the stream would have reached Naples and flowed into the sea. About one-third of Massa was destroyed, and less than one-fourth of S. Sebastiano. The stream which separates the two villages is more than half a mile in breadth and 20 feet in depth.

On the night of April 26th, 1872, the Vesuvius Observatory lay between two streams of lava, and the inmates found the radiated heat very oppressive. The lava seemed to ooze through the sides of the cone. The two craters which had opened near the edge of the great crater gave vent to loud bellowings, produced great clouds of smoke and ashes, and projected volcanic bombs to a height which Palmieri estimated at nearly 1500 feet. This would imply an initial velocity of 655 feet per second. The white ashes were carried by the wind as far as Cosenza. Dark-coloured sand, lapilli, fragments of scoriæ, and much smoke were at intervals emitted during the eruption. Many people fled in terror from Naples; Palmieri remained, however, at his post in the Observatory.

On the morning of April 27th the lava ceased to flow. It was noticed that in three several places in the lava bed great masses of smoke were emitted, and projectiles were driven into the air, as if small volcanic rents were existing beneath. But this effect, which sometimes lasted from fifteen to twenty minutes, was probably due to the sudden evolution of gases produced by the passage of the lava over wet places. On the evening of the 27th the pine-tree cloud of smoke from the crater became darker in colour, and frequent flashes of lightning were emitted. On the following day the pile of ashes and lapilli was so great that it darkened the air, and great terror pervaded the inhabitants of Resina,

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Portici, and Naples. On the 29th a strong wind blew from the crater towards the Observatory, and the scoriæ broke some of the windows. Ashes accumulated to a depth of 7½ inches on the roof. Towards midnight the noise of the craters diminished; the next day the detonations were few and far between; and by May 1st, 1872, the eruption was completely at an end.

On ascending to the summit of the cone Palmieri found the great crater divided into two parts by a wall with vertical sides 750 feet in height. The interior showed no sublimations, but alternate beds of scoriæ and compact lava. Fumeroles emitting sulphurous anhydride, hydrochloric acid, and sulphuretted hydrogen at a temperature of 150° C. were found at the outside edge of the crater.

Palmieri regards the great eruption of April 26th, 1872, as the last phase of the eruption which commenced early in 1871. He calculates that the total amount of lava emitted amounts approximately to 20,000,000 cubic metres, about three-fifths of which was deposited on pre-existing lavas. Its destruction was estimated at 3,000,000 of lire. Among the products of the eruption were noted:—Black oxide of copper, chloride of lead, chloride of iron, chloride of ammonium, chloride of calcium, and chloride of sodium. Traces of the chlorides of thallium and lithium were also detected.

For some months after the dying-out of the eruption of 1872 Vesuvius remained in a quiescent state, and the only manifest sign of the hidden fires appeared in the few fumeroles on the floor of the crater. These, however, gradually increased in number, the temperature rose, and steam and carbonic acid were abundantly evolved. Afterwards the carbonic acid diminished, and sulphurous acid made its appearance. Early in January, 1875, the writer ascended the mountain, and found the crater full of steam and sulphurous acid. It was quite impossible to descend into it. As the wind from time to time blew the fumes over the edge of the crater we suffered from a feeling of suffocation, and violent coughing was produced. One of our party wore a pea-jacket of fluffy serge dyed with indigo, and the sulphurous acid was in sufficient quantity to bleach it superficially.

Later in the year hydrochloric acid appeared—a sure sign that the volcanic activity was increasing. This gas is produced by the action of silicates at a red-heat upon salt in the presence of aqueous vapour, and is a constant product of intense volcanic action in all parts of the world. To-

wards the end of the year (December 18th) a deep fissure opened in the floor of the crater, at the bottom of which molten lava could be seen. The surging liquid mass afterwards rose to the top of the fissure, and a cone was thrown up over the point of greatest activity. From this lava flowed in small quantities and at intervals during 1876, 1877, and 1878. It overspread, and gradually raised, the floor of the crater, until, on November 18th, 1878, it rose to the edge of the lowest portion of the rim of the crater on the north-west side of the mountain, and ran down the great cone towards the Atrio del Cavallo. The flow soon ceased, but the new cone continued to exhibit considerable dynamic activity, and is still ejecting scoriæ and evolving volumes of steam.

When the writer visited the scene of the new eruption, a few weeks after its commencement (December 29th, 1878), the cone exhibited considerable activity. We ascended with a guide from Resina, and the ascent thence to the foot of the cone occupied an hour and three-quarters, while that of the cone itself—which slopes at an angle of 32°, and is composed of loose ashes—occupied fifty-five minutes. The cone on the north side presented precisely the same aspect as in 1875. On arriving at the rim of the crater we turned to the west, until we came to a portion which had been broken down by the recent flow of lava. Near this north-western extremity of the rim we turned aside, and descended to the floor of the crater by a very steep path. At the bottom of the crater we at once found ourselves upon the new lava, which in many places was red-hot a few feet beneath the surface. Dense fumes of hydrochloric acid escaped from fissures in the lava nearest to the new cone, and certain large cavernous places in the lava were coated with brilliant red and yellow incrustations of sesquichloride of iron—often erroneously described as sulphur. Masses of nearly white porous lava were sometimes found near the cone. These had probably been submitted to the action of hydrochloric acid at a high temperature, which had removed the iron from the mass in the form of chloride. Such decomposed masses consist almost entirely of silicates of lime and alumina: they may be artificially produced by passing hydrochloric acid over volcanic scoriæ heated to redness in a porcelain tube.

Palmieri has detected chloride of calcium and chloride of lithium among the products of the last eruption; also boracic acid and various sulphates. In specimens collected in December we have found, among other substances, chloride

of aluminium, and microscopic crystals of a compound of copper, probably chloride. The lava is black and lustrous, and it contains a good deal of leucite. It does not resemble that which was emitted in 1872.

The new cone of November, 1878, presents several points of great interest. It is so small, and its dynamic activity is of such a character, that it is possible to approach quite close to it, and thus to observe the exact nature of its action. We approached within a dozen yards of its crater, and stood nearly on a level with it on a heap of scoriæ, which sometimes shook beneath our feet. A peculiar loud surging sound was heard within the crater, such as we have heard within the Strokkr Geysir, in Iceland, but much louder and more intense—in fact the difference that one would expect between violently agitated water and violently agitated molten lava. At intervals loud detonations occurred, the ground shook under our feet, and a thousand red-hot pieces of scoriæ were shot on to a height of several hundred feet. The majority of these fall back into the crater, or on that side of the cone away from the wind; but sometimes, on the occurrence of an unusually violent outburst, the masses would be scattered far and wide. Two such masses fell unpleasantly near; the second, which must have weighed at least a pound and a half, fell within 4 feet of us. They can generally be avoided if they come down singly, but of course if a shower descends the best thing to do is to run for it. A guide had been killed by a falling projectile fifteen days before our visit. The new cone presented altogether one of the most remarkable sights we have ever witnessed; the earth-shakings, concussions, and detonations; the showers of red-hot scoriæ, rivalling any fireworks; the clouds of steam and lurid smoke, and the entire surroundings—all contributed to make it a remarkable sight, and to prove that a really great eruption, like that of 1631 or 1872, must be altogether the most wonderful sight in Nature.

In the last number of the "Bullettino del Vulcanismo Italiano," which was published a few weeks ago, we look in vain for any news of Vesuvius. In the "Cronaca Vesuviana" an account of our visit in December (communicated to "Nature," and very inaccurately translated for "La Nature") is reprinted with all the errors. Of the products Prof. Palmieri writes—"Le sublimazioni, peculiare obbieto di studi scientifici, sottoposte continuamente all' azione delle pioggie, non han potuto accumularsi in

gran copia. Tra' soliti cloruri e solfati si è confermato allo spettroscopio la presenza dell' acido borico e del litio." A correspondent of "La Nature" writes from Naples on January 24:—"L'éruption du Vésuve vient de recommencer. Ce soir a 7 heures 30 minutes, la coulée est superbe, la flamme plus vive et le torrent de feu plus animé qu'il ne l'a été pendant toute la période antécédent. . . L'éruption continue au grand cône; au sortir de ce qu'on appelle ici la 'Fenètre' la lave se déverse en deux courants dont l'un suit le lit de la coulée antérieure vers Somma, et dont l'autre se dirige vers Naples." No augmention of intensity seems to have taken place since the above was written, and the eruption which commenced last November still justifies Palmieri's appellation of *una modesta eruzione*.

A record of the history of Vesuvius during the last decade would be very incomplete without reference to a notable addition to the literature of the mountain, in the "Report on the Chemical, Mineralogical, and Microscopical Characters of the Lavas of Vesuvius from 1631 to 1868," by Profs. Haughton and Hull, printed in the "Transactions of the Royal Irish Academy" for March, 1876. Herein the authors have endeavoured to discuss the composition of the Vesuvian lavas as a branch of the Indeterminate Analysis, founded on what Prof. Haughton calls "the principle of least paste." These lavas are mainly composed of nine minerals, viz., leucite, plagioclase, magnetite, olivine, augite, hornblende, mica, nepheline, and sodalite, of known composition, together with an unknown quantity of a paste of unknown composition. The object of the authors is to group the constituents given in the ultimate analysis of the lavas into their several compounds; for example, to say how much of the silica exists in leucite, how much in augite, and so on. Of course it is well nigh impossible to say how the various constituents of lava are grouped in the form of different distinct minerals, but we can gain some idea of the abundance or scarcity of certain minerals in a lava by the microscopic analysis. In applying the principle ofleast paste Prof. Haughton has proceeded on the assumption that "of the numerous solutions possible that one will occur in Nature which involves the largest amount of Definite Minerals and the least amount of Indefinite Paste."

The following proximate and ultimate analyses will illustrate the result of Prof. Haughton's work:—

	Lava de Granatello. Vesuvius, 1631.	Lava Stalactite. Vesuvius, 1868.
Silica	48·54	46·58
Alumina...	14·86	20·00
Iron peroxide ...	4·17	3·20
Lime	11·89	9·09
Magnesia	5·75	3·16
Soda	2·71	2·74
Potash	6·45	7·35
Iron protoxide ...	4·82	5·69
Titanium protoxide	0·21	0·27
Manganese protoxide	1·18	1·07
Phosphoric acid ...	0·18	Not determined
Fluorine...	None	None
Chlorine...	Not determined	Not determined
Water	0·16	0·32
	100·92	99·47

The same lavas with the constituents grouped according to the principle of least paste :—

	Lava de Granatello.	Lava Stalactite.
Leucite	33·60	41·80
Anorthite	0·60	18·20
Magnetite	4·45	3·75
Olivine	trace	—
Augite	41·20	22·60
Hornblende	trace	—
Nepheline	10·00	—
Sodalite	trace	6·04
Apatite	0·44	—
Paste	9·71	7·61
	100·00	100·00

The percentage composition of the minimum paste in the case of the Lava di Granatello is—

Silica...	45·0
Lime	27·0
Protoxides...	28·0
	100·00

And of the other—

Silica	21·6
Lime	13·2
Iron protoxide	65·2
	100·00

The details of the calculations are given at length in the memoir, which is accompanied by a description of the microscopical examination of thin sections of the lavas by Prof. Hull, and a coloured plate giving the more marked peculiarities. We trust that the principle of least paste will hereafter be applied to the lavas of Etna, the Lipari Islands, Iceland, and Hawaii.

Addendum.—Towards the end of last month, when Etna was in a state of violent eruption, Vesuvius showed signs of an increase in dynamic activity. A telegram from Naples, dated June 11th, states that "A slight eruption has commenced to-day from Mount Vesuvius, and is gradually increasing." A private letter from Naples, dated June 16th, contains the last piece of news which we have been able to collect concerning the present condition of Vesuvius:—"It shows some small activity at the summit. The upper crater is full of liquid lava, which at times boils over, and sometimes also is thrown up, but no great amount of force is to be observed."—R.

III. A NEW THEORY OF DEW.

INVESTIGATIONS which Prof. Levi Stockbridge has made at the Amherst Agricultural College, upon the comparative temperature of soil and air, and the disposition of dew upon the earth and plants, have led him to conclusions very different from those commonly received in regard to the formation of dew. It is usually held that dew is the moisture of the air, condensed through contact with objects of a lower temperature, and that it does not form till radiation has reduced the temperature of the earth and other objects below that of the atmosphere. The experiments referred to seem to indicate that, as regards objects in the immediate vicinity of the earth, at least, the process is

the converse of this, viz., that dew is the result of condensation, by the air, of warm vapour as it rises from the soil. The course of experiments from which this novel scientific theory was deduced is outlined below.

The basis of the theory is the discovery that in summer the average temperature of the earth at night is higher than that of the atmosphere. The temperature of the earth in an enclosed space on a level with the surrounding soil, and the temperature of the air, were taken at the warmest time of day and the coldest time of night for several months, and the average temperature of the air for the season was found to be 72·940°, and that of the soil 72·061°. But the average temperature of the air at night was 49·664°, and that of the soil 56·370°, the earth thus averaging at night over 6° warmer than the atmosphere. The temperature of the soil and air at night was also taken at various points within 10 miles of the college, on all kinds of grass land and bare soil, and in the forest, and the same facts were obtained, the soil being at all times warmer at night than the air.

These results led to experiments on dew-fall. Two boxes, each of a cubic foot capacity, were filled with soil without disturbing its particles or disarranging its strata; one receiving absorbent, retentive loam, and the other peat. These boxes were placed in a trench, in an open field, level with the surrounding ground, and exposed to the weather. Through the month of June they were weighed night and morning, and unless there was a rain in the night they uniformly weighed less in the morning than at night, the loss being from 1 to 3 ounces for the loam, and 1 to 4 ounces for the peat. This, Prof. Stockbridge thought, indicated that the soil at night gave forth water, and that the moisture found on the surface of a field in the morning came from a deeper soil rather than from the air. Other similar experiments followed. In one a cabbage-plant was enclosed in an air-tight tin case. Where the stem of the plant protruded through the top of the case, wax was used to make it impossible for moisture to escape through the leaves. The can was first kept within doors and weighed night and morning, when it always showed a loss during the night of 1·21 to 1·78 grms. When left out-doors at night, with the can wrapped in cloths to prevent moisture reaching it, the loss was from 0·55 grm. to 4·23 grms., showing a loss even when there was moisture or dew on the leaves.

These experiments, continued through the season, gave Prof. Stockbridge these proofs of his proposition that the dew on the ground in the summer is the condensation of

vapour that rises from the earth:—1. The vapour of the soil is much warmer at night than the air, and would be condensed by it. 2. Vapour from the soil is soon diffused and equalised in the whole atmosphere, but in largest proportion when evaporation is taking place near the surface of the soil; and, other things being equal, plants nearest the earth have the most dew. 3. Dew under haycocks, boards, and like objects on the ground, could receive it from no other source.—*Kansas City Review of Science and Industry.*

IV. ARTIFICIAL FLIGHT.*

By F. W. Brearey,
Honorary Secretary to the Aëronautical Society.

IN the Paper which I read before the Aëronautical Society at the General Meeting in 1878 I stated that, with respect to those of my models which are actuated by wing-movement, I had found a difficulty of sustaining the weight which appertains to the living example. But from experiments which I have lately made I find that the weight to be sustained depends upon power and strength of material employed, and also upon the right application of that power. For instance, in the wing-action of the bird, that portion of the pectoral muscle which depresses the wing is considerably more powerful than that which elevates it, because its function is to sustain the whole weight of the bird by impact upon the air. In my models I attain that action by affixing india-rubber cord, more or less in tension (according to the power used), underneath the wings, so that when the wings are elevated there shall be a strong downward impetus. There is, however, a disadvantage not shared by the bird,—the upward stroke in the model absorbs much power in working against the tension of the india-rubber underneath. Given that power, and with material to endure the pressure considerable weight can no doubt be sustained, What immediately concerns us, however, is this:—Can the weight equal to that of a man and the additional power necessary to propel him be sustained by any material which man can construct light enough and strong enough?

* A Paper read before the Aëronautical Society of Great Britain.

Mr. Wenham, in his Paper read at the first meeting of the Aëronautical Society, says—"Having remarked how thin a stratum of air is displaced beneath the wings of a bird in rapid flight, it follows that, in order to obtain the necessary *length* of plane for supporting heavy weights, the surfaces may be superposed, or placed in parallel rows, with an interval between them. A dozen pelicans may fly one above the other without material impediment, as if framed together; and it is thus shown how two hundredweight may be supported in a transverse distance of only 10 feet." But I think that the difficulty of actuating the wings of these twelve mechanically imitated pelicans would present as great a difficulty as the vibration and construction of wings of the dimension of "60 feet from end to end, and 4 feet across at the widest part," which in the same Paper Mr. Wenham estimates as necessary to support the weight of a man. If to this we add the weight of motive power—for it is evident that man does not possess the necessary power—we might, but for other encouraging reasons, give up the hope of his ever being able to navigate the air, because its possibility, in the way that a bird flies, is held to rest either upon the necessary surface extended laterally or else superposed, and in either case—were the difficulty overcome—there would remain the important question of balance, which in the bird is maintained, I believe, similarly to that unconscious muscular action in the sole of the foot and leg, when a man stands in an upright position. Under such a surface, therefore, there would be, I maintain, no safety.

I grant that a light and very powerful motor would favourably alter some of these conditions, because the rapid vibration of a much smaller surface would effect the same result, but increase of strength of structure would be necessary.

I had hoped that some rotatory action, which would have presented to the air in rapid succession all the equivalents of flight by wing-surface, would have solved the difficulty; but the only attempt hitherto made in that direction has been effected by Mr. Moy, at a cost of several hundred pounds. The velocity which was requisite in its preliminary run was fatal to the attempt, because it could not be attained even if the machine had been effective on leaving the ground.

Then what has man to propose to evade these difficulties, which appear insuperable? Need I mention that he resorts to balloon propulsion in one of the various forms which are always recurring to minds who take freshly to the subject, and which are always supposed to be new.

Is there any need for me to dwell upon this negation of all hope for those who desire to navigate, and not to float and stagger about, in the air?

There remain, therefore, the advocates of the screw-propelled plane. For the lecture-room this is a very pretty and effective experiment, and I have propelled such for years with screws both fore and aft; but I have failed to see safety to life under such a surface, even were there not another objection, and that a fatal one, to their use.

I have always said that any aërial machine, to be safe, should have sufficient power to rise from the ground. It will then possess power to control its descent. But no plane has hitherto been devised capable of rising from the ground with any preliminary run which could be imparted to it. I have watched such experiments with much interest. Mr. Linfield's ambitious attempt failed to afford any indication of rising power, although he has travelled upon wheels at somewhere about 20 miles an hour, with a surface overhead of about 300 square feet set at an angle originally. The dimensions are—Length 40 feet, width 18 feet, height 15 feet. The weight of all, including Mr. Linfield, is 304 lbs.

Mr. Moy also tried with a model weighing 1½ lbs., also upon wheels, the screws actuated by twisted india-rubber, but he failed in my presence to effect any rise at all. He succeeded in attaining only 10 miles an hour.

The preliminary run with the velocity requisite for success seems to me to be fatal to any attempt of this nature—equally so any descent from an elevation—to attain the initial velocity.

The bird with extended wings does not rise by passively holding out his wings as a plane, but during his preliminary run he either slowly waves his wings or else tremulously vibrates them, which enables him to feel the air, and gradually to obtain a fulcrum upon it.

There is yet another group of inventors who hope, by presenting a suitable supporting surface to an accommodating breeze, to be lifted thereby, and then, by the aid of gravity and this friendly breeze, to sustain themselves like a kite, and even to progress when they get accustomed to the position.

For the last three years I have myself been a continuous experimenter, and one can scarcely work in any field of science without advancing a step in knowledge. Much more likely is the investigator to be rewarded if the field has been almost abandoned. Many a nugget has been dug out of a deserted claim. I will remark, however, in passing, that

experimenters in flight are placed at a great disadvantage. The laboratory, or anything of the dimensions of a laboratory, would suffocate flight. Space and privacy are two necessities. The absence of these greatly deters progress, because for experiments in flight the object must *fly;* and we are necessarily, most of us, confined to models of small dimensions, which upon an increased scale would have more than a proportionate effect.

There is a mode of progression adopted by some fishes which is not reproduced by any flying creature, and in observing that motion—which was an undulating one—I asked myself whether in the air it would act also as a support? The fish is about the same gravity as the water; but how would the undulating motion of a loose fabric support a weight in the air? The result of my experiment was so satisfactory that it seems to me to get over the great difficulties that always suggest themselves in all plans except our own. For instance, in some designs which possessed encouraging features there seemed a want of provision for a safe and gradual descent, so that a parachute naturally suggested itself as a necessary adjunct. But the addition of a parachute means weight, and also an incumbrance. In the loose material, however, which I employ, both for support and propulsion, there *exists* the parachute, which acts upon cessation of the motive power, and brings the machine down with a gently gliding motion in the direction of its travel.

In none of the plans which have been submitted to the Aëronautical Society has there been the slightest hint that flight could be attained by imparting a wave-like action to a loose surface extended in the direction of its length; nor do I well see how it would occur to anyone to try the effect, unless he had been experimenting with flying models, so that he could readily substitute one arrangement for another. Certain it is that this discovery enables us to make a very large surface effective for support, which with wings alone appeared to be impossible. Of course, like every machine intended to find support in the air, it must be balanced; but in the model which I have made, such is its stability that an inch more or less does not affect it. So that we are at liberty to contemplate the construction of an aërial vehicle whose dimensions would suffice to maintain in wave motion six or seven hundred square feet of canvas, actuated by steam-power, and capable of supporting the additional weight of a man, whose weight, together with the machine, would certainly not exceed 500 lbs.; and we can contemplate the man as being able to move a few feet backward or

forward without much affecting the stability of the machine. His descent under the parachute action can thus be graduated at will. This can also be effected by a cord attached to the tail, which by that means can be elevated or depressed at pleasure. Placed upon wheels it has, of course, yet to be ascertained what distance of preliminary run would be required, assisted by the action of the fabric, before it would rise from the ground.

In Mr. Linfield's construction I see the framework required. The alteration would be flexible arms, and I think a looser arrangement of the cloth. The treadle action might or might not be able to vibrate the wing-arms. These flexible arms I construct of a bundle of canes, about 12 or 14 feet long, bound tightly together with whipcord throughout their whole length, and cutting off a cane every 2 feet or so in order to taper them. The wing-arm then affords lightness combined with strength, for it is almost impossible to fracture it. In this case we should be spared the weight of the screw, which is something considerable.

It has to be determined by experiment what velocity and what arc of vibration is best suited to the weight to be sustained. There will be also some relation between the arc of vibration of the wing-arm and the amount of surface in undulation.

In the model which I have made there exists a strong illustration of the assertion that any device made in miniature which will fly, can, when constructed upon a scale of utility, perform not only all, but much more than all, the promises extracted from it in the model form. In the first place, the experimenter is greatly in want of some motive power which will last sufficiently long to allow of close observation. Now in the recoil of the india-rubber which I have employed to produce the revolution of the crank which vibrates the arms, there exists only power sufficient to give a dozen strokes or so. And were I to increase the throw of the crank so as to increase the arc of vibration at the end of the wing, I should require so much greater power that either I could not with my own exertions wind it up, or else the parts would fail to bear the strain. For want of that continuous power I cannot say what amount of weight the model which I have made will carry, but I know *this* to a certainty, viz., that the weight to be carried depends upon the power and the strength of material for a given surface kept in a state of vibration. The weight of the larger model is 3 lbs. 1½ ounces, of which 7 ounces is added weight, which it easily carries. The dimensions are 6 feet wide by 10 feet

long, with about 16 square feet of surface. By this arrangement of loose waving surface I have had the smaller model (5 × 8) fly from my hand perfectly horizontally to the extent of 60 feet.

What has assured me that there is a necessary relation between the arc of vibration, the velocity, the weight, and the surface, is the fact that through breakage I had to review the parts, and I have never since regained that chance relationship; but the model appears to descend until the loose fabric is seen to hold the air, so that if the ballast is behind the centre of effort the surface is forced outward at an angle of perhaps 10°. Upon stoppage of the motive power there would then be no parachute action, but the machine would descend tail first. The ballast, however, properly arranged, the velocity of the model is increased, and the head being heavier, and power ceasing, onward motion continues under the concave extension of the surface.

All this would be at the command of a man with a power upon which he could depend. It is probable that if this wave-action were used in addition to a wing about a foot wide, a greater impact upon the air at starting might assist the supporting effort.

I perhaps need scarcely say that the judgment must be formed not upon the duration of flight or the distance travelled, but upon the effect observed in any part of its short flight which with my limited power I am enabled to give the model. It then becomes evident that we should only require a continuance of the power.

I think that after fourteen years' study of the subject by the members of the Aëronautical Society we are now in a far more favourable position than we have ever yet been to form a fair estimate of the probabilities of success.

The conditions necessary for flight are pretty well understood. There remain but the mechanical difficulties. How far those difficulties have now been minimised can only be satisfactorily determined by actual experiment upon a scale commensurate with the importance of the subject. And with respect to the model which illustrates this wave-action we may predicate for it that—largely constructed with power, strength, and surface—there would be as much difference in the effect as between a child's halfpenny toy, with four paper vanes pinned to the end of a stick, and the windmill whose larger vanes grind our corn.

V. BIRDS'-NESTING.

By ALLEN HARKER.

AS the time of year again comes round when the nesting operations of birds begin, it may not be unseasonable to make to our readers some plea on behalf of our native birds, and to indicate to all who may have influence to exert, or authority to exercise, on the question of birds'-nesting, methods by which alike the study of Ornithology and the interests of our bird population may be best served, and their preservation insured.

Surely of all God's creatures which delight us, those which give us the purest, most unalloyed pleasure, are the

"Feather'd songsters of the grove"!

How much of the charm of English or Scottish landscape, of deep wooded lane and open breezy heath and common, is due to the presence of ever active, ever musical bird-life, may be best appreciated by a visit to countries where the birds are songless, or where, as in many parts of the Continent, for great stretches of country, scarce a bird is to be seen. Such a visit could not fail to impress the lover of Nature with a sense of his duty to use his endeavours to preserve from the dull monotony of a birdless country the melodious fields and hedgerows of rural England.

It is not solely on behalf of the birds themselves, and their nests and eggs, that it seems desirable to awaken greater interest and action, but also in favour of a more systematic and accurate study of the subject, combined with a true scientific use of the knowledge thus obtained—knowledge which is now in a purposeless manner allowed to become lost.

The building of a bird's nest, the choice of site, the selection and gathering of materials, the deft skill that carries on and perfects the work; the number, size, and colour of the eggs, their variations, the period of incubation, the sex of the incubator, the condition of the new-born young, their growth, and first essays at independent flight—these are events in the natural history of the bird, no less interesting or important to the ornithologist than its anatomy, its distribution, or any other of the chapters in its history. But

in addition to their interest to the ornithologist, these events have ever possessed a special charm and interest for the young of all ranks of life. The beauty of the nest, and the beauty of form and colour of the egg, accompanied too by the novelty of finding such a treasure, combine to present a temptation to the finder too strong for any schoolboy to resist. The desire of possessing coloured and shining objects, which he possesses in common with the savage whose adult condition of mind he represents, leads to the destruction by him of a vast number of eggs and nests; for the youth, having taken the prize, speedily tires of his acquisition, and the eggs are soon broken, and new "sensations" sought for.

Most people will agree that such wanton destruction is to be deplored; but for ages birds'-nesting has been the schoolboy's license, and while most parents disapprove of it, few go the length of absolutely prohibiting it—still fewer use it as a means of imparting that pleasing knowledge of the natural history of birds which would soonest cure the propensity, and teach the youth at once humanity and zoology. That some efficient protection should be given to eggs and nests is the unanimous feeling of naturalists.

At present the parent birds themselves are protected during the season of nesting by a somewhat imperfect—but still, as far as it goes, a very beneficent—Act of Parliament; but it need hardly be pointed out that if a female bird is not allowed to rear any young, the chances against it (personally or by its offspring) surviving the winter, or, if it is a migratory bird, of returning in the following spring, are increased by as many times more as the number of young it might be supposed to rear; and in this way persistent and injudicious birds'-nesting may soon diminish the number of birds in a given area. And that birds'-nesting has this effect will not be doubted by any one whose study of the subject has led him to remark the diminution of small and rare birds in particular districts, or by one who has had opportunities of knowing what immense numbers of birds' eggs are annually destroyed by marauding youths.

There are always at work enough of causes—some natural, some artificial—which are not preventible, tending to diminish the numbers of our birds, more especially of our smaller song birds, either by cutting off their food supplies or by destroying their nesting grounds. Every field, marsh, or swamp that is drained, lessens the supply of insect-life on which a great majority of small birds live; every piece

of land that is reclaimed from waste robs the ground or low bush nesting birds of their *habitat;* every wood that is cut down, every gorse-patch that is burned,—in short, every advance of cultivation,—drives before it some species of birds.

It was my fortune to revisit, after a lapse of ten years, a part of the country where some of my earliest birds'-nesting exploits had been carried out. "High-farming" had taken the place of a more primitive agriculture; the thick high hedges where red-backed shrikes, bullfinches, linnets, and long-tailed tits were wont to nest, were supplanted by neat trim-cut hedges 3 feet high, and not thick enough to offer cover for the smallest of birds. The deep ditches with high grass-grown banks, once the haunt of wood wren, lesser whitethroat, or whinchat, had disappeared, and patches of gorse and heather, where redpole and linnet once dwelt, had been burnt and stubbed out long ago. These causes, which for the birds' sake we may deplore, we cannot nor should we wish to prevent; and even consolation is to be found in that while one species of bird may be driven out, another suited to the new condition may follow and take its place. The richest arable land are especially the resort of the lark, who dispels the monotony with his "sweet jargoning." It is rather with preventible causes that we have to deal; and to the indiscriminate and utterly wanton birds'-nesting, for no intelligent or intellectual aim or object, which goes on in every parish in the country, a check must be applied. Here it is that the authority of parents and schoolmasters should come into force. It is in most cases due as much to *ignorance* as to wantonness or destructiveness that the youthful birds'-nester takes eggs for which he has no use—no idea of use, in fact; they serve to gratify his instinct for finding and possessing pretty objects, and then are strung on a string as an ornament, or made cockshies of as an amusing pastime. Had he been taught anything of the importance of the nest and eggs to the continuance of the parent birds,—or had any facts of their history, as of how birds differ from other animals, or how, in a sense, a nest and eggs are as much a part of the mother as the embryos of viviparous animals,—his nesting for pure wanton destruction of his spoil would at least be checked; or if he proved not amenable to such reasoning, should be forcibly prevented and heavily punished. But a remedy for checking birds'-nesting in the intelligent boy who wishes to avoid wantonness, but at the same time claims a right to make his

collection of eggs as much as another collector has one to make his of butterflies or birds, must be sought in another direction, and is worth the consideration of lovers both of birds and of intelligent and inquiring schoolboys.

In passing it must be remarked that schoolboys alone are not to be blamed for purposeless birds'-nesting, and it would be unfair to pass over as great a culprit, the amateur adult collector, whose condemnation should be as much greater as is his opportunity of knowing better. It has been well remarked that few, if any, advances have been made by human beings in their history, but have been accompanied by a concomitant development of special vices, originating in a perverted application or use of the benefits gained by the advance. The form which this aberration assumes in connection with the rise and progress of biological science is as a mania for amassing large collections of animal structures, whether shells or birds' eggs, or the animals themselves, without any reference whatever to their structure or history, or to the educational purpose they might serve, when this latter exists at all. When you see in the drawers of a collector of birds' eggs a long series of the eggs of the kingfisher or a wild duck, not one egg in each series differing in any way from another, the inane purposelessness of the thing—not to use any stronger term—is evident. No clearer proof could be given that the great majority of egg-collecting—that is, by others than schoolboys—arises from a barbarous desire of possession alone, than that it is seldom if ever accompanied by the collecting of birds' *nests*, from which probably much more is to be learned of bird history than from the eggs. I need only refer to the learned observations of Pouchet on the changes which he remarked in the building of the nests of species of *Hirundo*, to instance the interesting and important results which a study of nests might lead us to.

Compare the nest of a chaffinch with those of its congeners the greenfinch or the bullfinch, or that of a sedge warbler with that of the wood wren; how totally different they are—one feels inclined to say, comparing incomparables, more different than the birds themselves. Or to go further, compare one chaffinch's nest with another, and note the variety in material, and even in construction, adapted to some peculiarity of situation or surroundings. Here is a field for observation and comparison by means of which the philosophical student may hope to catch some

glimpses of the working of the laws which have taught birds, as they must in time have been taught, to build nests in endless variety of form and material. Little, if anything, has been written beyond mere speculation on this subject, which presents so much scope for investigation. In spite, too, of all the vast collections of birds' eggs which have been made, we are still almost entirely in the dark as to any theory, even, of the causes which have tended to produce such infinite variety of form and colour for what is really but for one and the same end.

The method of, at one and the same time, limiting the tendency to purposeless egg-collecting, and systematically extending our knowledge of the whole subject, which it is the purpose of this paper to suggest, is the formation in connection with local museums, or, where these do not exist, with larger schools, of complete educational collections of birds' nests and eggs. Under the judicious guidance of the head-master this might be done with but a minimum of wrong to the parent birds in at most three years, and the eggs and nests which would be required to complete such a collection would be many times fewer than what are annually destroyed in the same area, and would, by being carefully housed and attended to, obviate, as I shall endeavour to show, the necessity for repeated nesting in time to come.

Many years ago I assisted at the formation of such a collection for a small country museum, and our method of preserving and displaying it to the best advantage being devised chiefly with a view to economy of space and material, it may be useful to briefly describe it. We took our nests generally before any eggs—or but the first—had been laid, and binding them carefully with tape or cord we saturated them or sprinkled them with some preservative liquid (methylated spirits and corrosive sublimate solution is best), to kill any vermin and keep away moths in the future, and then dried them and packed them in drawers or boxes. The eggs we got when we could. Our collection being nearly complete, we had a wall case about 5 feet high and 12 inches wide, made with shelves sloping at an angle of 45°. On the bottom shelf we placed the large nests, such as those of the crow, rook, jackdaw, magpie, &c., putting into each nest the number of eggs usually laid, not necessarily, and indeed seldom, taken from that particular nest. On the second shelf, such nests as consist of little more than a few reeds or grasses, or a mere hollow scraped

in earth, were contained in round card-board boxes, to hold the materials together. These included the nests of the curlew, sandpiper, gulls, coot, water-hen, ducks, grebes, &c. Smaller boxes held the collection of bones on which the kingfisher lays its eggs, the few grasses that the skylark lines a hole in the soil with, and so on. Upper shelves held the smaller nests; and when they had been built in holes of trees we cut away the branch, where possible. In time we had not only a very complete collection, but an additional case of such eggs as vary from a common type, and our little museum was the favourite resort of all the schoolboys in the district; and it is certain that much less bird-nesting for mere nesting's sake was one outcome of our efforts. Sometimes we had brought us varieties which were not found in our cases—a result which was most desirable and pleasing, as it evidenced an intelligent appreciation of the uses of a collection. A schoolmaster who will but inaugurate such a collection will certainly be conferring a lasting benefit on his pupils, and sparing many a nest from careless destruction. Certain it is, too, that if every country town and village had its small, well-ordered, local museum, where the common objects of animal and plant life—which ever will possess an absorbing interest for the young—were displayed in such a manner as to convey some intelligent ideas of their life-history and relation to each other, among the innumerable benefits which would accrue, one if not the least would be that there would be created a sympathy between the animals and their keen-eyed observers, and the wanton destruction of myriads of them would be diminished.

One of the most pleasant recollections of a short residence in a French country town is associated with its admirable museum, and the character of its visitors on Sunday afternoons. On week-days the students from an *école de medecine* shared with me the examination of its well-arranged cases; but on Sunday afternoons troops of peasants and their families took the place of the systematic student, —the husband in his clean blouse, the wife in clean starched cap of marvellous and stupendous proportions, and the children in clean Sunday best. The shouts of the youngsters at the wonders of strange foreign *bêtes* were only passed by their delight at the discovery of old friends like *M. Crapaud* or *les petites papillons bleues;* while older schoolboys conferred together over cases of beetles or bottles of reptiles, or related, in audible whispers, exploits with beast

and bird which the preserved specimens recalled to mind. For two or three hours the rooms were filled with these happy-faced students of zoology; and the remembrance of Sunday afternoons nearer home, spent by British peasants in less creditable ways, rose, not unnaturally, in the mind, with the reflection that "they manage these things better in France."

In forming such a collection of birds' eggs and nests as I have described, with the desire, at the same time, to collect with such discrimination and judgment as to limit to a minimum, if not to avoid entirely, cruelty or injury to the birds, there are certain broad maxims which must be held in mind; and it is these which it is my object to strongly impress. It is well established by experience that many birds will go on laying eggs in the same nest after the loss of their first eggs; and, physiologically, there exists the most ample provision in the mother bird for such a contingency. Others will build new nests again and again after the destruction of their first efforts; but manifestly there is a limit, if only in point of time and season, to these persistent efforts at propagating their kind. No egg collector should, therefore, ever take eggs or nests after a certain date,—say, 1st of June,—except in the case of very late migrants. This limiting date should, of course, vary with different species of birds, and in different parts of the country, but should be fixed and rigidly adhered to.

A second maxim should be, that no collector should ever take partially incubated eggs, or disturb the nest in such case; and lastly, that he should never take any egg or nest at all that is not intended to form part of some new public collection, or to supply a blank in such already-established one. The common possession of a perfect collection, which might thus be speedily formed, would not only have the influences which experience has proved similar ones to have on schoolboys, but with "schoolboys of a larger growth" birds and their nests and eggs would become more familiar and interesting objects, and the ranks of ornithologists be swelled by new and devoted students. Furthermore, with such opportunities of comparing observations and discovering variations in habits of nesting, or in the phenomena connected with birds' eggs, the study of birds' nests and eggs might in time be raised to the dignity, which it scarcely at present occupies, of a science. Vast as is the number of facts which we are in possession of regarding the subject, the time scarcely seems to have arrived when these may be

formulated, and a theory of birds' eggs be established. The interesting researches of Mr. Sorby into the nature and composition of the colouring-matter of birds' eggs belong rather to the sciences of chemistry and spectroscopy than to those of oology or animal physiology. What has been done towards an elucidation of the subject may form matter for future consideration. If this appeal should induce one lover of birds to take any steps to form a permanent and accessible record of their habits of nidification, it will have more than served its object.—*The Scottish Naturalist.*

NOTICES OF BOOKS.

Evolution, Old and New; or the Theories of Buffon, Dr. Erasmus Darwin, and Lamarck, as compared with that of Mr. Charles Darwin. By SAMUEL BUTLER. London: Hardwicke and Bogue.

THE work before us appears to have a threefold object. The author undertakes the rehabilitation of certain Evolutionist worthies of days bygone—possibly too much neglected and even misinterpreted—the refutation of the hypothesis of Natural Selection, as put forward by Mr. C. Darwin and by Mr. A. R. Wallace; and, lastly, the exposition of his own peculiar views on the development of species. Mr. Butler does not appear to be a working biologist, nor indeed a man of science at all. Still, unlike the bulk of outsiders who have written upon Evolution, he rejects the old hypothesis of Special Creation, and if his contentions do not everywhere command our implicit assent, they cannot fail to supply every true naturalist with abundant matter for profitable reflection.

It is generally, and perhaps too hastily, assumed that the Doctrine of Development, as met with in the works of Buffon, Erasmus Darwin, and Lamarck, was so crude and imperfect as to necessitate its rejection by men of sound and sober judgment, whilst Mr. C. Darwin, by supplying his idea of Natural Selection, has converted the chaos into a cosmos, and rendered the adoption of Evolutionism not merely possible, but almost imperative. Mr. Butler, on the contrary, holds the very contrary opinion:—"Fresh from the study of the older men, and also of Mr. Darwin himself, I failed to see that Mr. Darwin had 'unravelled and illuminated' a tangled skein, but believed him, on the contrary, to have tangled and obscured what his predecessors had made in great part, if not wholly, plain. The older men, if not in full daylight, at any rate saw in what quarter of the sky the dawn was breaking, and were looking steadily towards it." Again we read—"Those were the days before 'Natural Selection' had been discharged into the waters of the Evolution controversy, like the secretion of a cuttle-fish." One of Mr. Butler's charges against "Darwinism" is that, shutting out purpose and intelligence from the work, it refers the formation of species to the accumulation of small and hap-hazard variations—an objection which has been urged by Mr. J. J. Murphy and by Mr. Mivart. A not less capital point is that Natural Selection, though it may preserve any variation that proves advantageous, has plainly nothing whatever to do with the origin of such variation. This

will at once appear if we reject the words "Natural Selection," and write in their stead the expression "Survival of the Fittest," which Mr. Darwin himself declares to be equivalent in meaning. Everyone will then admit that before the fittest can survive it must come into existence. Here Mr. Darwin leaves us in the dark, giving us an "Origin of Species" with the "Origin" cut out. But this is not all: the bulk of the readers of Mr. Darwin's work go away with the impression that Natural Selection is the most important cause of modification. Now Mr. Darwin certainly protests against being understood to assert that Natural Selection induces variability, and yet, perhaps from inadvertence, uses language open to a very different interpretation. Thus he declares Natural Selection the most important "means" of modification. Here Mr. Butler remarks—"'Means' is a dangerous word; it slips too easily into cause. We have seen Mr. Darwin himself say that Buffon did not enter on 'the *causes or means*' of modification, as though the two words were synonymous or nearly so. The use of the word 'means' enables Mr. Darwin to speak of Natural Selection as if it were an active cause (which he constantly does), and yet to avoid expressly maintaining that it is a cause of modification."

It will now be asked, what does Mr. Butler offer us instead of Natural Selection as a more satisfactory solution of the mutation of forms and the rise of species. Taking up the views of Lamarck and of the elder Darwin, he contends that "differentiations of structure and instinct are due to the desires under changing circumstances of an organism, which must be regarded as a single creature, though its development has extended over millions of years."

Between parents and offspring there is oneness of personality—a view which explains the phenomena of heredity and of instinct, or, as it might be called, unconscious memory. Purpose and intelligence are thus brought into play in the genesis of species, but the intelligence is not that of an Allwise Creator, which cannot be supposed as taking tentative steps, or abandoning its plans as unsatisfactory and working in a different direction.

With reference to the nidification of birds, we find a case quoted from Miss Seward's biography of Erasmus Darwin which supplies the crucial experiment asked for by Mr. Wallace in his Essay on the "Philosophy of Birds' Nests."* A pair of young canaries, who had never seen a nest built, are described as constructing their nest on the slovenly regulation pattern of their race, "even to the precise disposal of every hair and shred of wool." For such a fact Mr. Butler's theory accounts without the invocation of a mysterious "instinct."

Into the biographies of Buffon, Erasmus Darwin, and Lamarck, and into the author's exposition of their views, space does not allow us to enter. We can only quote two passages, very different

* Contributions to the Theory of Natural Selection, p. 211.

in their scope, but equally suggestive :—" Science is not a kingdom into which a poor man can enter easily if he happens to differ from a philosopher who gives good dinners, and has 'his sisters and his cousins and his aunts' to play the part of chorus to him."

" For our skull is as a kind of flower-pot, and holds the soi from which we spring,—that is to say, the brain ; our mouth and stomach are roots in two stories or stages ; our bones are the trellis-work to which we cling; *we* are the nerves which are rooted in the brain, and draw thence the sustenance supplied by the stomach; our lungs are leaves folded up within us, as the blossom of a fig is hidden within the fruit itself." We are thus likened to " perambulating vegetables turned upside down."

" Evolution, Old and New " is well worth reading."

A Manual of Organic Chemistry, Practical and Theoretical. For Colleges and Schools, Medical and Civil Service Examinations, and especially for Elementary, Advanced, and Honours Students at the Classes of the Science and Art Department, South Kensington. By HUGH CLEMENTS, of H.M. Civil Service; President of the Amateur Mechanics' Workshops Association, London ; and Lecturer on Various Sciences at St. Thomas Charterhouse and several other Science Institutions in London. London : Blackie and Son.

THIS Manual is in great part a reprint from the " English Mechanic," and is unquestionably an examinational work. It contains chapters on the distinction between organic and inorganic substances ; on organic analysis and empirical formula (*sic*) ; on the determination of rational formulæ ; on organic substitution ; on the theory of compound organic radicals ; the preparation and properties of their hydrides ; the distillation of coal ; on alcohols, ethers, aldehyds, organic acids, anhydrides, ketons, alkaloids, and organo-metallic compounds. Then follows a section on the identification of organic compounds ; and a description of oils, fixed and essential. We have next an account of certain articles of apparatus, a selection of exercises, and a list of " Papers set in Organic Chemistry at the Examinations of the Science and Art Department from 1868 to 1878, with Answers," —a section which occupies nearly one-third of the entire book, and which may give the student, or rather the intending examinee, some idea of the class of questions likely to fall to his lot.

That the bulk of the matter contained in the work is on a level with the knowledge of the day is unquestionable. Still we come here and there upon passages which, to us at least, seem to require emendation. Thus the author tells us that as carbonic

anhydride "is a product of the respiration of animals, it is decidedly an organic product, and should therefore be included with organic compounds"—an argument which we can scarcely admit. Blood receives a formula $C_{45}H_{40}N_6O_{15}$, as though it were a definite chemical individual. Yeast, we learn, "uninfluences it" (*i.e.*, gum). Human bile "has a nauseous fragrant odour."

On p. 50 we have, *verbatim et literatim*, the following passage:—"Picric or trinitrophenylic acid, like any acid, forms salts that crystallise well. It has the odour of tar or creosote, and has a hot taste. It is much used as a disinfectant, rendering sewage, &c., inoffensive. It quickly arrests all fermentative and putrefactive changes, &c." Now the chemist who knows the properties of picric acid will here call a halt, and, looking back, perceive that what is said concerning disinfecting and antiputrescent properties must refer not to picric acid, but to phenol, of which the author has been previously speaking. But the student who may have no previous knowledge of picric acid and of phenol, and who can merely follow the grammatical construction of the passage, will here, we submit, be led somewhat astray.

Speaking of fats Mr. Clements says—"The fat of warm-blooded animals is generally solid, while that of cold-blooded animals *and* fish is liquid." Fish are generally supposed to be "cold-blooded animals," but this sentence shows that our "lecturer on various sciences" is of a different opinion. He continues—"The principal solid fats are butter, grease, lard, and suet, and the fluid animal fats are cod-liver, neats' foot, sperm and whale oils." It is curious that, of the fluid fats enumerated, all, save one, should be the products of warm-blooded animals.

These peculiarities, which are not the only ones that might be selected on close scrutiny, show that the "Manual of Organic Chemistry" needs revision before it can be pronounced a trustworthy guide.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 4, and Vol. v., No. 1. Washington: Government Printing-Office.

THE first of these two numbers contains an account of the fossil insects found in the Tertiary shales of the Green River, Wyoming, by that well-known entomologist S. H. Scudder. The collection, eighty species of which have been identified, is remarkable for the distinctly tropical aspect of its forms. Dr. D. S. Jordan reports on the collection of fishes made in Dakota and Montana by the indefatigable Dr. Elliott Coues; whilst Prof. J. W. Chickering furnishes a catalogue of phænogamous and vascular cryptoga-

mous plants collected by the same naturalist in the above districts. Mr. F. M. Endlich's paper on "Erosion-products in Colorado" would prove a most instructive study for those—and such writers exist—who deny that erosion has any share in the formation of the earth's surface. Dr. C. A. White discusses the palæontology of the "Laramic group," and Mr. J. A. Allen gives a synonymatic list of the American *Sciuri*, or tree-squirrels.

The first number of Vol. v. contains much interesting matter. Dr. C. V. Riley and Mr. J. Monell contribute a monograph of the American Aphididæ—a group of insects of loathsome aspect, but interesting from their peculiar system of reproduction, and from the ravages they commit in the fields and gardens. The first annual generation of *Schinoneura Americana*, the American elm Aphis, consisting entirely of females, issues from impregnated eggs which have survived the winter, and without sexual intercourse produces a second generation like themselves in structure. The third generation is winged. The fourth and fifth again are wingless, the sixth winged, whilst the seventh consists for the first time of individuals of both sexes. Each female deposits a single egg, which is not hatched till the ensuing spring. Amongst the natural enemies of this species the author enumerates several Coleoptera, especially Coccinellidæ, two Hemipterous species, and even the larva of a moth (*Semasia prunivora*). The misfortune is that the enemies of Aphides are, in point of number and voracity, utterly inadequate to the task set before them.

Prof. E. D. Cope discusses the relations of the horizons of extinct Vertebrates of Europe and North America. The author points out, as a consequence of the principle of descent, that the types of each age have taken their origin from the generalised types of preceding ages, there being no descent from the more specialised types. Hence in discriminating the subdivisions of geological time we have to be guided by the disappearance rather than by the appearance of species. He finds that portions of all the faunæ of all the primary divisions of geologic time have been recognised on both continents,—that parallels requiring general identification of the principal divisions of these faunæ may be detected, but that exact identifications of restricted divisions may be made in a few instances only. Between the tables of extinct animal and vegetable life there is a discrepancy. The plant-life of North America reached its present condition one epoch earlier than did the higher Vertebrata. Hence either the animal-life of North America has lagged behind that of Europe, or the flora of America has been in advance of the European. The author mentions that the earliest land vertebrates had a persistent *chorda dorsalis*.

This number further contains papers on the miocene fauna of Oregon; on the birds of Dakota; on the fossils of the Jurassic trias of south-eastern Idaho and western Wyoming; on the fossilised forests of the volcanic tertiaries in the Yellowstone

National Park; on the conditions of preservation of invertebrate fossils; and a supplement to the bibliography of North American invertebrate palæontology.

The Journal of the Cincinnati Society of Natural History. January, 1879. Vol. i., No. 4.

In this number we have a paper by Mr. V. T. Chambers on the tongue of certain Hymenoptera. The tube in this organ is in the hive-bee 1-500th of an inch in diameter, but in some of the Andrenidæ only half that width and closed at the apex. It is curious that though Fritz Müller and Wolff, as well as the author, have demonstrated the tubular character of a bee's tongue, it should be described as solid in the latest edition of the "Encyclopædia Britannica."

The most important article, however, is a very elaborate list of the birds occurring in the Cincinnati district. We shall not be surprised if, in spite of the "British" mania prevalent among ornithologists, entomologists, and botanists in this country,—a whim which leads to little save the extirpation of our rarer forms, the range of each native species will be mapped out accurately in America sooner than in these islands.

We learn, with little surprise, that the introduction of the European sparrow into the United States is now looked upon by competent authorities as a deplorable mistake. We hope that other parts of the world, not yet suffering from the presence of this "winged rat," will take warning from the experience of our American friends. Acclimatisation is not a matter to be rashly undertaken.

Euclid and his Modern Rivals. By Charles L. Dodgson, M.A., Senior Student and Mathematical Lecturer of Christ Church, Oxford. London: Macmillan. 1879.

Mr. Dodgson's drama, or rather dialogue, opens with a soliloquy from "*Minos*," an examiner tortured by having to look over papers in Geometry, containing a mixture of the old and new systems. Minos, after some conversation with "*Rhadamanthus*," another equally unhappy examiner, falls asleep, and to him appears the shade of Euclid ready to do battle for his Manual. A discussion follows, in which the reasons for *retaining* Euclid's Manual are treated, but it is noticeable that those for rejecting it are only just mentioned; and even that is denied to many.

In fact, throughout his book, the author is far more inclined to attack Euclid's Rivals than to defend Euclid himself, so that a one-sided view of the question is shown, and Euclid's faults are kept steadily in the back-ground. Euclid and Minos then discuss the method of procedure in examining Modern Rivals, and a few alterations proposed by them.

In the Second Act Minos is discovered still sleeping, and to him enters a shade which is addressed as "*Niemand*," with copies of the Modern Rivals to be discussed. These are divided into two classes—those which reject Euclid's treatment of parallels, including Legendre, Cooley, Cuthbertson, Wilson, Pierce, and Willock; and those which adopt Euclid's treatment of parallels—these are Chauvenet, Loomis, Morell, Reynolds, Wright, the Syllabus of the Association for the Improvement of Geometrical Teaching, and Wilson's Manual founded on that Syllabus.

In the Fourth Act the shade of Euclid again converses with Minos, and, with the exception of some one or two slight additions and alterations, such as a proof that all right angles are equal, it is decided that Euclid's Manual ought to be left untouched.

The chief points of attack on Euclid's Modern Rivals are Mr. Wilson's two works—"Elementary Geometry" and the "Manual founded on the Association Syllabus." The author makes comparatively short work of Legendre's book as unsuited to beginners, though doubtless valuable to advanced students; and of Cooley's, in which a certain theorem breaks down through a faulty definition of parallel lines. But to Mr. Wilson's two Manuals he devotes nearly a third of his volume. Much of the criticism on these, however, is mere cavilling; for instance, at page 177, Minos says, speaking of Wilson's "Syllabus" Manual:—"At p. 57 I see an 'Exercise' (No. 5). '*Show that the angles of an equiangular triangle are equal to two-thirds of a right angle.*' In this attempt I feel sure I should fail. In early life I was taught to believe them equal to *two right angles*—an antiquated prejudice no doubt; but it is difficult to eradicate these childish instincts." This is mere straw-splitting; *strictest* accuracy would of course require the insertion of "*each*" before "equal," but if the sum of the interior angles had been intended to be understood "*together*" before "equal" would have been absolutely necessary. The very next paragraph, containing an accusation of the fallacy *Petitio Principii*, is another instance of cavilling criticism; for to take a line greater than half another, surely we need only take it greater than the whole, and that involves no knowledge of the bisecting point of the line.

At page 160 there is a criticism on the definition of a right angle as given by the Association for the Improvement of Geometrical Teaching in their Syllabus. This is—"When one straight line stands upon another straight line, and makes the adjacent angles equal, each of the angles is called a right angle."

XUM

Since the Association Syllabus admits of angles equal to or greater than two right angles, this is open to the objection that it does not debar the case in which one line stands on the *end* of the other, making the adjacent angles equal to one another, and to *two* right angles as right angles are generally considered. That is certainly a grave objection, but the same applies equally to Euclid's definition, or else a proof must be supplied, which is not that in the case mentioned the two lines are in one and the same straight line; and so this interpretation is debarred by Euclid's limiting clause.

In fact, though Mr. Dodgson's book is interesting and often witty, he fails to prove his point, because he takes a one-sided view of the question, and merely exhibits the blunders of Euclid's Rivals without balancing them against Euclid's own. Besides which—though perhaps it is more readable than an essay—a dialogue does not seem to be the clearest form for setting forth arguments and facts. It is certainly an advantage that the more solid part of a treatise of this sort should be enlivened here and there by lighter matter, more especially as it is so managed as not to break the thread of the argument.

The Tables of Problems—though there is a somewhat complex system of notation to be mastered before they become intelligible—are extremely interesting; more especially Table II., which is described as "consisting of theorems admitted to be real and valid, but *not* deducible from undisputed axioms."

CORRESPONDENCE.

THE SENSES.

To the Editor of the Monthly Journal of Science.

SIR,—In an article which you inserted not very long ago the possible existence of other senses than the five with which we are personally familiar was suggested. You will perhaps therefore permit me to call your attention to a lecture by Professor Pierce, of Cambridge (U.S.). He points out that auditory vibrations are not more than twenty thousand per second, whilst the slowest visual vibrations are four hundred millions of millions. Between these two limits, therefore, there is a range sufficient for more than forty additional senses, "each of which might have its own peculiar effect upon the nerves of the observer, and give a corresponding variety of information." Surely this consideration alone ought to convince us of the vast extent of our own ignorance and of the folly of pronouncing on the non-existence of whatever escapes our scanty means of research.—I am, &c.,

CAUTION.

HEREDITY.

To the Editor of the Monthly Journal of Science.

SIR,—A recent writer explains the heredity of talents, character, and instincts by the hypothesis that the young animal is a continuation of its parents, and enters upon life with a remembrance of their actions, which, though latent, is roused into activity by the force of circumstances. Hence the young bird, when the breeding-season comes on, builds a nest on the pattern followed by its ancestors. This assumption offers an excellent explanation of the peculiar type of nest adhered to by each species, and even of the architectural peculiarities of the solitary wasps, bees, &c., who have never even seen their parents. But I find a difficulty as regards the song of birds. Experience shows that a young bird brought up without the society of its own species will never adopt their characteristic note, any more than a child reared among wild beasts—of which there have been cases—will

speak the language of its parents. Now if a bird is a continuation of the personality of its ancestors, why should it not adopt their song as instinctively as their style of nest-building.—I am, &c.,

AN OLD NATURALIST.

THE BAND PATTERN IN ANIMALS.

To the Editor of the Monthly Journal of Science.

SIR,—In reply to J. W. S. permit me to say that the facts he mentions respecting the band pattern in animals have often been observed. In a recent number of "Science for All" (vol. i., p. 252), Mr. W. Ackroyd remarks that a bilateral symmetry of marking exists in the head of the Bengal tiger and bodies of zebra, the Indian tapir, the Aard wolf, and some of our domestic cats. He then proceeds to adduce many facts which seem to point out that colour uniformities—*i.e.*, sexual differences, bilateral symmetry, and colour distribution generally—are "regulated by some deeply-seated and symmetrically distributed portion of the organism, such as the nervous system.

I may observe that the bands are often parallel to the axial line, there being many examples of this in the British Museum. May we not, therefore, look upon the band pattern as of three kinds with respect to the axial line:—1st, the right-angle pattern; 2nd, the parallel pattern; and 3rd, what may be taken as a mixture of the two—the spotted pattern?—I am, &c.,

L. J. DE WHALLEY.

June 23, 1879.

PROCEEDINGS OF SOCIETIES.

ROYAL SOCIETY, *May* 1.—"On the Origin of the Parallel Roads of Lochaber, and their bearing on other Phenomena of the Glacial Period," by Joseph Prestwich, M.A., F.R.S., F.G.S., &c., Professor of Geology in the University of Oxford. Of the various hypotheses that have been brought forward since the time of Macculloch and Dick-Lauder to account for the origin of the parallel roads of Glen Roy, the one so ably propounded by Mr. Jamieson, in 1863, has been most generally received and adopted. It is a modification of the views originally expressed by Agassiz, to the effect that the barriers of the lakes—to the shore action of which both the above named geologists attributed the "roads," but were at a loss to account both for the formation and removal of barriers—had been formed during the Glacial period by glaciers issued from Glen Treig and Glen Arkaig, supplemented by others from Ben Nevis. The subsequent determination, by the Scotch geologists, of an intermediate milder period succeeded by a second cold period, led Mr. Jamieson to conclude that the extension of these two glaciers took place during the second cold period, which he thinks was of little less intensity than the first, and that, while the glacier from Glen Arkaig blocked up Glen Gluoy, the glacier from Glen Teig formed a barrier to Glen Roy. This and other objections are considered by the author to be fatal to the hypothesis advanced by Mr. Jamieson; but while objecting to this exposition of the Glacial theory, he considers that that theory affords the most satisfactory solution of the problem, only that he would suggest a different interpretation in explanation of the phenomena. Dismissing the hypothesis of local glaciers of the second period of glaciation, the author falls back upon the original idea of Agassiz with the development acquired by more recent research, and assigns the Lochaber lakes to the close of the first period of great glaciation. The general conclusions drawn by the author from the phenomena in Lochaber and surrounding district are—

1st. That at the period of the first great glaciation of Scotland the ice-sheet in Lochaber attained a thickness of not less than 2000 to 2500 feet, but that in consequence of the peculiar physiographical conditions of the district the large ice-currents from Ben Nevis so clashed with others in the Spean and Lochy Valleys that a block ensued which led to an exceptional heaping up and accumulation of the ice in front of Glen Roy and Glen Gluoy.

2nd. In consequence of the lowering and partial submergence of the land, and its conversion from a continental area to an archipelago, combined possibly with some other more general cause, an amelioration of the climate took place, attended by a gradual melting of the ice-sheet in all the lower tracts. The snow and ice wasted from the valleys and from the lower mountain summits, and, in the absence of any established water-courses, the hollows and depressions in the ice, when not fissured, were converted into pools and tarns, until the continued liquefaction opened out surface channels or interior fissures, by which the water could ultimately escape.

3rd. That pending the establishment of natural lines of drainage, and in presence at places of unusual obstruction, the water accumulated in some valleys in larger bodies or lakes; and if in those cases, the mouth of the valleys being closed by the main ice-barriers, other channels of escape—such as cols or passes—communicating with adjacent glens or valleys presented themselves, the water overflowed through these channels as soon as it rose to the height of such cols or passes. Or should the cols have been also barred by ice, that ice would have given way as soon as the increasing height and pressure of the water proved sufficient. When this happened the water would at once fall to the fixed level regulated by the col, and thus no record, such as we have in Lochaber, might be left of the presence of the original bodies of water.

4th. In the Lochaber district, while the exceptional accumulation of ice in the Spean Valley barred the entrance of the glens on the north side of that valley, their passes were also blocked by remnants of the great ice-sheet; and the formation of the detrital shelves is due to the sudden bursting of these minor barriers, when the waters of the lake were discharged with great rapidity, until they fell to the level of the col. Under these circumstances the mass of loose *débris* covering the hill sides below the line of water-level gave way, and slid atter the retreating waters, until stayed with greater or lesser abruptness, according to the angle of slope and the volume of the mass, on the discharge ceasing and the waters coming to rest. The shelves so formed, modified slightly by subsequent subaërial action, constitute the "roads."

5th. The moraine detritus in places where the glaciers clashed, and where their progress consequently became checked or delayed, tended also to accumulate or heap up, and in this way in the Lochaber glens added to the strength and permanence of the ice-barriers.

6th. While the moraine detritus was irregularly distributed under the ice, or massed in particular places, the *débris* projected on the surface of the ice-stream, and contained in its body, was either left *in situ* on its liquefaction or else was—as the result of the great floods consequent upon the bursting of lake barriers—

carried successively to lower levels, leaving here and there banks of sand and gravel at various heights on the hill sides. These destructive floods, combined with the incessant river inundations due to the same general thaw of the great ice-sheet, carried down and spread out in the valleys and plains the great beds of gravel and sand which—with the modifications since brought about by long-continued fluviatile action—have given rise to various forms of escars, terraces, and other less-defined accumulations of these detrital materials.

May 15.—"Some Researches with Professor Hughes's new Instrument for the Measurement of Hearing—the Audiometer," by Benjamin Ward Richardson, M.D., LL.D., F.R.S.* The Audiometer consists of two Leclanché cells for the battery, a new and simple microphonic key connected with the cells and with two fixed primary coils, and a secondary or induction-coil the terminals of which are attached to a telephone. The induction-coil moves on a bar between the two fixed coils, and the bar is graduated into 200 parts, by which the readings of sound are taken. The graduated scale is divided into 20 centims., and each of these parts is subdivided into 10, so that the hearing may be tested from the maximum of 200 units to 0^{o} — zero. The fixed coil on the right hand contains 6 metres of wire; the fixed coil on the left hand contains 100 metres. By this means a long scale from the left-hand coil is produced. The secondary coil contains 100 metres of wire. In using the instrument, one Leclanché cell has been found sufficient, as a general rule; but two have been used in instances where the hearing of the person under test has been very defective. The person whose hearing is being tested should sit in an easy position, and should not see the act of the observer in moving the microphone key. For good observation the room in which the experiment is made should be large, and all external causes of sound—such as the ticking of clocks, the vibrations of windows and doors, the moving of feet, and the singing from gas jets—should be silenced. The sitter should close the ear that is not applied to the telephone while he is listening for minute sounds, and should give his full and calm attention to the proceeding. The instrument may be considered to afford the most satisfactory means for testing the hearing power of all persons who can define a sound. The range of sound is sufficient at the maximum, 200^{o}, for everyone who is not absolutely deaf 0^{o}, or zero, is a point of positive silence from the instrument, or rather from the sound which it produces through the telephone. One of the first facts learned with the audiometer is the suddenness with which the sound is lost to those who are listening. The sound is abruptly lost within a range of 2^{o}; that is, within one-hundredth part of the

* For a description of Prof. Hughes's induction-currents balance see p. 508.

entire scale. This is the case with those who are very deaf, as well as with those who hear readily. The effect of filling the chest and holding the breath makes a difference in listeners. The capacity for hearing is for a few seconds increased by holding the breath. Holding the breath with the chest not full fails to produce the same result. As a rule, the hearing of persons who are right-handed is most refined in the right ear, and as most persons are right-handed it is found that the right ear is the best ear. This rule is, however, attended with many exceptions, since, for various reasons, some persons who use the right hand exclusively, practise for some particular purpose the use of the left ear, upon which that ear becomes more acute. Connected with the last-named fact is another, namely, that by this instrument the deaf are found to fail in capacity of hearing not only by reason of physical defect, but also by failure of memory of sounds. Thus in a youth who had suffered serious defect of hearing for seven years, owing to partial destruction of the tympanum, and who in the right ear could only detect sound at 107°, there was an inability to catch all the sound lying between 130° and 107°, until he could remember what he had to listen for. By practising him then to detect the lowest sound that he was physically capable of receiving, the author got him to detect this one sound more readily than those which came higher up. By further practice all the intervening sounds became audible with equal facility. These facts, which have been confirmed by another observation on a different person, seem to indicate that deafness from imperfection of the tympanum or other parts of the organ of hearing may be increased, beyond the mere physical failure, either from some lost power of automatic adjustment in the auditory apparatus or from failure of receptive power in the cerebrum itself, so that the memory rendered imperfect is slow to assist the listener until by exercise of function its readiness is restored. By use of the audiometer the influence of atmospheric pressure on hearing is detectable. In the author's own case, when the barometer is at 30° he can hear on both sides close down to zero, but below 30° he fails by 2° on the left side to reach zero. Dr. Richardson has tried to determine in some of the lower animals whether there is the same sense of hearing as in man. In most animals it is difficult to obtain sufficient quietude to enable the observer to gather from expression or movement of the animal the information sought for. In two dogs—one a terrier, the other a field spaniel—he succeeded in making some good observations, and in them the range of hearing power seemed to be distinctly lower than it is in the human subject who has perfect hearing. In both these animals, which were healthy and in the prime of life, the first indication of the detection of sound commenced at 10° on the scale. The detection was evidenced by the sudden expression of listening, by a slight change of position, and a slight dilatation of the

pupils. This detection was clearly made on the instant, as if the sharp line of hearing were the same in them as it is in the higher animal.

The following practical memoranda are given by the author:—

"1. The audiometer will, I think, be an essential in all physical examinations of men who are undergoing examination as to their fitness for special services requiring perfect hearing, such as soldiers, sentries, railway officials, and the like.

"2. The instrument will be of great use to the physician in determining the value of hearing in those who are deaf, and in determining the relative values of the two organs of hearing. In one instance, already, I have been able by its means to detect in a person who was supposed to be equally deaf on both sides, that on one side the hearing is perfect close up to zero, while on the other side nine-tenths of the hearing is lost.

"3. In other forms of diagnosis I have found the instrument useful. In a young person suffering from acute anæmia the hearing was so defective that on the right side it failed to detect sound at 18°, and on the left side at 15°. In ten days, during which, under a new regimen, great improvement took place in strength and general condition, the power of hearing had so much improved that the right ear was good down to 12°, and the left to 3°,—an improvement of 6° on the right, and of 12° on the left side. In another person, who was subject to repeated vertigo, the giddiness occurring three or four times a-day, the hearing was so defective that although the external ear on each side was clear and the tympanum natural, no sound could be heard below 30°. Under complete rest and attention to diet the vertiginous attacks were in a few days removed altogether, and with that removal there was gain of hearing, on both sides equally, up to 5° on the audiometric scale.

"4. The instrument may be used to differentiate between deafness through the external ear and deafness from closure of the Eustachian tube—throat deafness. In my own case I fail to detect sound by the mouth at 170°, and this I find is a fair average in those who are healthy. It represents the comparative value of communication by sound through the Eustachian canal and the external ear.

"5. The instrument promises to be very useful in detecting the effects in the body of those agents which quicken or excite the circulation, such as alcohol and other similar chemical substances.

"6. The instrument promises to be of great service in determining the value of artificial tympanums in instances of deafness due to imperfection or destruction of the natural tympanum. The cotton artificial tympanums introduced originally by the late Dr. Yearsley, and the membranous tympanums introduced by the late Mr. Toynbee, F.R.S., have proved of much service; and by means of the audiometer I have been able very accurately to test their respective merits, and to compare both with tympanums

made of other material. The inquiry has led me to test different metals for this purpose, and to find in fine gold the substance for making the most useful and effective artificial drum."

"Note on the Invention of a Method for making the Movements of the Pulse audible by the Telephone: the Sphygmophone," by Benjamin Ward Richardson, M.D., LL.D., F.R.S. While experimenting with the audiometer it occurred to me that I might get a secondary or telephonic sound from the movements of the pulse at the wrist. I have effected this in a very simple manner, by adding a microphone to a Pond's sphygmograph. I mount on a slip of talc a thin plate of platinum. I place the talc slip in the sphygmograph as if about to take a tracing of the pulse. I connect one terminal from a Leclanché cell to the slip of platinum on the talc, and the second terminal from the cell to a terminal of the telephone. Then I connect the other terminal of the telephone with the metal rod of the sphygmograph which supports the talc. The instrument is now ready for use. It is placed on the pulse, in the ordinary way, and is adjusted, with the writing needle thrown back, until a good pulsating movement of the needle is secured. When the movement is in full action the needle is thrown over to touch the platinum plate, which it traverses with each pulse-movement, and completes the connexion between the telephone and the battery. The needle, in passing over the metallic plate, causes a distinct series of sounds from the telephone, which correspond with the movements of the pulse. When all is neatly adjusted the sounds heard are three in number, one long sound and two short, corresponding to the systolic push, the arterial recoil, and the valvular check. The sounds are singular, as resembling the two words "bother it." The sounds can be made very loud by increasing the battery power. This little instrument is not so good a recorder of the pulse as the sphygmograph, but it may be made very useful in class, for illustrating to a large number of students, at one time, the movements of the natural pulse and the variations which occur in disease. I call the invention the Sphygmophone.

"Note on a recent Communication by Messrs. Liveing and Dewar," by J. Norman Lockyer, F.R.S. In a paper of last December the author called attention to the importance of discussing Young's observations of the chromospheric lines in connexion with the spectra of the metallic elements. Since his paper was read Messrs. Liveing and Dewar have given a table which professes to state the number of times various lines in certain metals were seen by Young in connexion with certain reversal phenomena observed by themselves. The statements, however, made in this table with regard to the visibility of certain lines in the chromosphere do not appear to the author to be in accordance with Young's published tables, and he considers that a higher degree of accuracy than that employed by Messrs. Liveing and Dewar is necessary to determine such coincidences.

The author's eleven years' work on this special branch has led him to the conclusion that all statements of coincidences between metallic and solar lines with a lower degree of accuracy than that employed by Thalén and Young are to be avoided when possible, as they may be worse than useless—they may mislead. Although the map on which he was working was on twelve times the scale of Angström's, it would have been better if it were larger; and in saying this he adds his tribute of admiration of the accuracy of the work of those who have preceded him, notably Angström, Thalén, Cornu, and Young, with whose work he is more familiar, as it is expressed in wave-lengths.

May 24.—"Note on the Spectrum of Sodium," by J. N. Lockyer, F.R.S. I have lately been engaged in studying the spectrum of sodium under new experimental conditions. In anticipation of a detailed communication I take leave to state that the vapour given off from the metal after slow distillation in a vacuum for some time shows the red and green lines without any trace whatever of the yellow one. Hydrogen is given off in large quantities, and at times the C line and the red "structure" are seen alone. After this treatment the metal, even when red-hot, volatilises with great difficulty.

May 29.—"Researches on Explosives. No. II. (Fired Gunpowder)," by Captain Noble, late R.A., F.R.S., F.R.A.S., F.C.S., and F. A. Abel, C.B., F.R.S., V.-P.C.S. The authors preface this memoir by two tables: one of these gives the results of some analyses of products of explosion of the three services powders of Waltham Abbey manufacture—pebble, R.L.G., and F.G.—which are required to complete the series of results obtained by firing the charge in different spaces, which were given in their first memoir on this subject, and a statement of the mean percentage composition by volume of the gases and by weight of the solid products furnished by those three powders, together with the highest and lowest proportions in which each product has been obtained with the three descriptions of powder. This table includes the results of examination of the products of explosion of four descriptions of powder differing in many respects from the powders chiefly employed in their researches. The other table gives the complete results of the whole series of analyses made, showing the proportion by weight of each solid and gaseous product, and including the amount of water pre-existent in the various specimens operated on. A portion of this memoir is devoted to a discussion of a few points raised in reference to the first memoir of the authors,* on fired gunpowder, by General Morin and M. Berthelot, who were appointed by the Académie des Sciences as a Commission to report on

* Phil. Trans., 1875, Part 1.

that memoir. In discussing M. Berthelot's views respecting the objections which the authors raise against the acceptance of any chemical equation as giving even a general idea of the metamorphosis which a gunpowder of average composition may be considered to undergo when exploded in a confined space, they disclaim having had any intention to convey the impression, which indeed they consider that their expressions were not calculated to convey, that it was impossible to put into *some* form of equation a representation of a variety of reactions which, if assumed to take place simultaneously, among different proportions of the powder-constituents, might give approximate expression to the results obtained in any one particular experiment, and might, thus far, afford some approach to a theoretical representation of the metamorphosis of gunpowder. What they desired to point out and lay stress upon was the conclusive proof, which is afforded by the very great variations in composition, of the solid portions more particularly, of the products of explosion of samples of gunpowder presenting only small differences of composition (and even of the products furnished at different times by one and the same sample), that the reactions which occur among the powder-constituents are susceptible of very considerable variations, regarding the causes of which it appears only possible to form conjectures, and that, consequently, "no value whatever can be attached to any attempt to give a general chemical expression to the metamorphosis of gunpowder of normal composition." An examination of the results of the author's experiments on the heat generated by the combustion of gunpowder shows, first, that the heat thus generated is subject to very wide variarions, dependent upon the particular nature of the powder employed (the Spanish powder, for example, generates just 50 per cent more heat than the mining powder); and, secondly, that the heat evolved by the same description of powder varies in different experiments to a greater extent than is to be accounted for by errors of observation. The authors' views on this head are confirmed by calorimetric determinations in their researches on gun-cotton. In these determinations, which have been carried on with precisely the same apparatus, no appreciable difference was found in the heat evolved in the various experiments. It is of high importance to observe that the volume of the permanent gases generated is in every case in inverse ratio to the units of heat evolved, as is shown by the following table.—

Nature of Powder.	Units of Heat per Gramme exploded.	Cub. centims. of Gas per Gramme exploded.
Spanish pellet powder ...	767·3	234·2
Curtis and Harvey's No. 6	764·4	241·0
W.A.F.G.	738·3	263·1
W.A.R.L.G.	725·7	274·2
W.A. Pebble	721·4	278·3
Mining	516·8	360·3

The results given in this table are very striking. Taking the two natures of powder which commence and close the list, the heat generated by the Spanish powder is about 50 per cent higher than that generated by the mining powder, while the quantity of permanent gases evolved by the latter is about 50 per cent greater than that given off by the former. Thus it appears that the great inferiority of heat developed by the mining powder, as compared with the Spanish powder, is compensated, or at least approximately so, by the great superiority in volume of permanent gases produced. A similar relation is observed in respect to the other powders, and it would indeed appear that the pressures at any given density and the capacity for performing work of the various powders are not very materially different. This fact has been entirely verified for the whole of the Waltham Abbey powders, and in a less degree for the three other powders also. The peculiarities shown by the mining powder are so interesting that it appeared important to determine its tension when fired under a high gravimetric density. 11,560 grs. (749 grms.) of this powder have therefore been fired under a gravimetric density of unity. The pressure developed by two very accordant observations was, when corrected, 44 tons on the square inch (6706 atmospheres). The pressure obtained under similar circumstances from Waltham Abbey powder was 43 tons on the square inch (6554 atmospheres), The capacity for performing work of the various descriptions of powder was also found to be not very different, a similarity of result the more remarkable seeing that with, at all events, three of the powders there are striking differences both in their composition and in the decomposition they experience, and when in consequence material variations both in pressures at different densities and in potential energy might have been expected. With respect to the great difference in heat evolved by the Spanish and mining powders, it appears difficult to resist the conclusion that the small number of units of heat evolved by the latter is in great measure due to the quantity of heat absorbed in placing the very much larger proportion of the products of combustion in the form of permanent gases. This suggestion would also appear fully to explain the fact alluded to in the authors' first memoir, and to which they had been led purely by experiment, namely, "that the variations observed in the decomposition of gunpowder do not, even when very considerable, materially affect either its tension or capacity for performing work." A comparison between different gunpowders, or a comparison between gunpowder and other explosive agents, cannot therefore, as has been proposed, be determined by a simple measurement of the corresponding units of heat they evolve. As regards the *actual* temperature of explosion, the results of the further experiments detailed in this paper leave little doubt that the temperature named in the authors' first memoir, viz., 2200° C., is not far

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removed from the truth for the principal powders with which they then experimented. The authors then discuss the constants in the equation, expressing the relation between the tension of the products of explosion and the volume these products occupy, as stated by them in their first memoir, and give values of those constants, corrected from the analyses and experiments made since the publication of that memoir, concluding their remarks upon this part of their subject with a table which gives in terms of the mean density of the powder products the tensions which would exist in the bores of guns were perfectly dry powder of normal composition suffered to expand, with or without production of work. The tensions are expressed in kilogrammes per square centimetre, tons per square inch, and atmospheres. The authors call attention to the great utility of a table (XI.) showing the theoretic work which a charge of gunpowder is capable of effecting in expanding to any value, v. The table given by them exhibits the theoretic work for all necessary volumes of v, from $v=1$ to $v=50$. The authors then discuss the causes which in the bore of a gun affect the energy realised by gunpowder, and point out that this energy varies very much with the powders employed, being in this respect dependent upon circumstances, such as the density of the powder, its size of grain, the amount of moisture, chemical composition, nature of charcoal used, &c., but that the energy may also vary considerably, even with the same powder, if the charges be not fired under precisely the same circumstances. For example, especially with slow burning powders, the weight of the shot fired exerts a very material influence upon the factor of effect, and the reason is obvious—the slower the shot moves at first, the earlier in its passage up the bore is the charge entirely consumed, and the higher is the energy realised. The same effect, unless modified by other circumstances, is produced when the charge is increased with the same weight of projectile. In this case the projectile has to traverse a greater length of bore before the same relief due to expansion is attained. The higher pressures which consequently rule react upon the rate of combustion of the powder, and again a somewhat higher energy is obtained. Both these increased effects, of course, correspond to an increased initial tension of the powder-gases, but, especially with the smaller guns, a very great difference in the realised energy may arise from other causes. The authors draw attention to the effect of retaining the shot in its seat for a greater or less time, thus giving rise in the former case to a more perfect and earlier combustion of the charge. They cite experiments to show that from this cause alone differences of energy in guns having a calibre of 12 centims. of 13 to 14 per cent have been obtained. It is pointed out that from the principles laid down it is possible, if the maximum chamber-pressure be known, to fix, very approximately, the position of the shot in the bore when the combustion of the charge

may practically be considered to be effected. The authors have little doubt that the main theories upon which they insist, confirmed as they are by experiments made or facts obtained, under very great variety of circumstances, may be accepted as, at any rate, close approximations to the truth. It is satisfactory to find that the laws which rule the tensions and temperatures of gases under ordinary circumstances do not lose their physical significance, but are still approximately applicable, at the high temperatures and pressures they have been considering. At all events, it appears certain that the rules and tables they have laid down, as based on their analyses, experiments, and calculations, may for all practical purposes be accepted as correct, and may, bearing in mind the restrictions to which they refer in this memoir, be applied to nearly every question of Internal Ballistics.

June 19.—"On the Production of Coloured Spectra by Light," by Capt. Abney, R.E., F.R.S. Last year I incidentally mentioned in a note to the Royal Society ("Proceedings," vol. xxviii., p. 291) that the production of natural colours by the agency of light, examples of which were shown by Becquerel, was probably caused by the oxidation of silver compounds employed. I have ventured to return to the subject, in order to show that the colours are so produced and are not due to interference. I have sent, for the Society's inspection, pictures of the solar spectrum on silver plates, and also on compounds of silver held *in situ* by collodion. It will be observed that the spectrum has imprinted itself in approximately its natural colours; that on the silver plates it is more brilliant than on the collodion film, but that in the latter the colours are seen by transmitted as well as by reflected light. I reserve for the present the exact details of the production of these pictures, but may say that they are produced by oxidation of silver compounds when placed in the spectrum, an exposure of two minutes being amply sufficient with a wide slit to impress the colours. The colouring-matter seems to be due to a mixture of two different sizes of molecules of the same chemical composition, one of which absorbs at the blue end and the other at the red end of the spectrum, and the sizes of these molecules are unalterable whilst exposed to the same wave-lengths as those by which they were produced. I believe it possible and probable that the colours may be preserved unchanged when exposed to ordinary daylight.

Meteorological Society, *June* 18.—Mr. C. Greaves, F.G.S., President, in the chair.

"Thermometer Exposure—Wall *versus* Stevenson's Screens," by William Marriott, F.M.S. It being the practice of some observers to expose their thermometers on walls facing north, it seemed a suitable object of inquiry whether instruments so

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placed gave results comparable with those obtained from thermometers in a Stevenson stand in the open. A pair of Meteorological Office wall-screens were fixed to the brick wall of an outhouse with a northern aspect, so that the screens were in the shade except in the morning and afternoon of the summer months. The Stevenson screen was on a grass plot 17 feet square, and about 50 feet north of the wall-screen. The Paper contains the results of the comparison of the maximum and minimum temperatures in the wall-screen with those in the Stevenson stand for the twelve months ending 31st March, 1879. The figures show that the mean daily maximum temperature on the wall is below that in the open, the monthly differences varying from 0·0° to −2·1°, that for the twelve months being −1·0.° The minimum temperature on the wall was mostly higher than in the Stevenson stand, the differences varying from −0·1° to +1·3, the mean for the year being +0·5°. The individual differences, however, are sometimes much greater, the maximum temperature on the wall being considerably lower than that in the stand. For instance, the difference exceeded 4·0 five times in September and four times in March, the greatest being 6·7°: these extremes occurred on fine calm days. The minimum temperature on the wall was more than 2·0° higher than that in the Stevenson stand on five occasions in June, seven in July. and four in September. The mean daily range of temperature on the wall for the twelve months was 1·4° less than in the stand in the open. The greatest difference was on March 9th, when the range on the wall was 8·5 less than on the stand. These results seem to show that, although the mean temperature may be roughly ascertained from thermometers shaded by a wall with a northern aspect, this method of exposure affords less sensitive indications than those obtained from instruments in a properly exposed Stevenson stand.

Physical Society, *May* 24, 1879.—Prof. W. G. Adams, President, in the chair.

Mr. W. J. Wilson exhibited a new harmonograph and figures drawn by it. The figures drawn by prior harmonographs are all more or less imperfect, owing to loss of motion in the pendulums actuating the marking pen; and Mr. Wilson therefore designed a new instrument, which not only gives perfect figures, but admits of the phase of either of the two compounded motions being decreased by a known amount. In this instrument toothed wheels take the place of pendulums, the ratio of the teeth giving the ratio of the periods of the motions. By employing the device of an intermediate wheel gearing with two others, and arranging two or more wheels on the intermediate axle, a great variety of phase can be obtained for each motion. An ingenious adjustment by means of a movable nut allows the phase of either or both motions to be altered to a known extent, and thus an

endless variety of figures can be obtained. As in other harmonographs, a writing table, on which the paper is placed, and an aniline glass pen, are used. Several of the figures done also on glass were thrown on the screen, the stereoscopic effect being very apparent. In reply to a query, Mr. Wilson said that he had adapted some of the figures to the stereoscope.

Prof. Hughes explained his new Induction Balance, and showed some of its principal effects. It is well known that on an electric current passing along a wire adjacent to another wire, an induced current is set up in the second wire in an opposite direction to the first or primary current. In the induction balance two primary circuits or coils are taken, with the same current (interrupted by a microphone acted upon by the ticking of a clock) running through both; and between these is placed a secondary circuit or coil in connection with a telephone. The primary coils are so wound as to oppose each other's induction on the intermediate secondary. There is a point, moreover, between where these opposed inductive influences exactly neutralise each other. If the secondary coil be placed there no induced ticking of the clock can be heard; but if the secondary be displaced to one or other side of this point the ticking can be heard in the telephone, increasing in loudness as the secondary approaches one or other of the primaries. If the distance between the primaries be graduated into a scale this contrivance becomes a sonometer, since it gives an absolute zero of sound and degrees of loudness. It is well adapted for measuring the hearing power of the ear, and when used for this purpose it is known as the audiometer. By splitting the secondary coil into two parts and placing each close to its proper primary, so that there are four coils in all arranged in two pairs, the extremes in one primary circuit and the means in one secondary, the two opposing parts of the balance can be separated from each other, so as not to disturb each other's action, and the balance made very sensitive by the closeness of the primaries and secondaries. Prof. Hughes finds that there is a line or zone of maximum induction midway between each primary and its secondary. If a conductor, such as a piece of metal, be put in this position it has a maximum distributing effect on the balance, due probably to the electric currents generated in it by the induction. The effect is apparently proportional to the conductivity of the metal. It requires an exactly similar piece of metal put between the other pair of coils to restore the equillibrium of the balance. A very slight difference of alloy or of weight between two like coins is at once observed from the imperfect restitution of the balance; base coins are thus also at once detected. Again, it is possible for a person to tell what particular coin or coins are in one part of the balance by trial of the same coins in his part. When plates of non-magnetic metals are held vertical in the balance their distributing effect is nil; iron, on the other hand, gives its maximum effect at this

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position, because its maximum effect overbalances its electrical effect. Two pieces of iron may therefore neutralise each other as cores in an induction coil. Steel is easy to balance compared with soft iron. Zinc disturbs most when placed along the sides of each pair of coils ; iron when in centre. A certain length of metal laid along the outsides of the coils produces silence. The maximum line of inductive force is midway between the coils, and there is a line of minimum force at half the thickness of each coil ; a metal placed at these lines of minimum force has no disturbing effect on the balance. Pressure, torsion, heat, magnetism, strain, and in fact all imponderable forces, are indicated, and their effects may be measured.

Prof. W. G. Adams believed that one result of Prof. Hughes's experiments will be the determination of the way in which the law of electro-dynamic induction depends on density. He also imagined that the metal when in the maximum line between the coils gathered the lines of force to itself, whereas when on the minimum lines it could not thus divert them.

Prof. Ayrton cited the early experiment of Faraday with a sheet of copper oscillating to rest between two opposite magnetic poles. The copper took a long time to stop ; but a sheet of iron placed between two like poles was soon stopped, owing to its becoming imbued with an opposite polarity, and deflecting the lines of force. He also suggested that the divergence of the results for conductivity of metals got by the induction balance from those got by the Wheatstone balance might be due to that electric inertia suspected by Sir William Thomson.

Prof. Guthrie thought that the induction balance pointed to the conclusion that the disturbing effect of a conducting mass applied in this way is proportional to the quantity of electricity generated in it under certain conditions of temperature, &c. The determination of the conductivity of liquids would be a useful application of the balance.

Mr. Chandler Roberts gave some results which he had obtained from an examination of certain alloys by means of the induction balance. He had been able to detect a difference of 1 part in 1000 in the amount of silver present in two shillings of equal weight. He also pointed out that Matthiessen divided alloys into three classes :—(1) Solidified solutions of one metal in another ; (2) Solidified solutions of one metal in an allotropic modification of another metal ; (3) Solidified solutions of allotropic modifications of both metals. For the first class the curve of electric conductivity is a straight line ; for the second, a parabolic curve ; for the third, a bent line. Mr. Roberts found that the balance gave the characteristic curve for the first class with an alloy of lead and tin, and for the second with an alloy of gold and silver. With a copper-tin alloy, which is a good example of the third class, he found the curve given by the balance to be intermediate between Alfred Rich's curve of density and Matthiessen's curve of conduc-

tivity, and considers that the balance is influenced by the density as well as by the conductivity of the metal interposed.

Prof. Hughes said that as the working of metals appeared to effect their conductivity, he was inclined to rely more on the conductivity measurements of the balance than on those of the Wheatstone bridge. By the balance plain pieces of metal were taken, whereas by the bridge wires were mostly taken. He would rather not give any theories yet as to the results obtained from the balance.

Dr. Erck then exhibited his novel pump for lifting solutions out of batteries. It consists of a closed vessel, funnel-like, the stem dipping into mercury, a column of which ascends the latter to a certain height. Two tubes emerge from the cover, one dipping into the liquid, the other opening to the air. By altering the pressure inside the vessel the solution rises to the latter, and can escape from it by trickling through the mercury.

NOTES.

Biology.

Some interesting results on the hereditary transmission of artificial injuries have been obtained by M. Brown-Séquard. He concludes that the young of parents abnormally constituted inherit external lesions, but not the central anomaly which determines such lesions.

M. Geoffroy has recently discussed the views of Magnus on the evolution of the colour-sense. He agrees substantially with Mr. A. R. Wallace and Mr. Grant Allen that it was not the perception of colours which was wanting in Homer's days, but merely a precise nomenclature for shades.

M. Zolyet has laid before the Society of Natural Sciences, at Bordeaux, some researches on the respiration of fishes. He finds that of all animals their respiration is the least active; they never, however, absorb more carbonic acid than they give off oxygen. Between the temperatures of 2° and 30°, which may be taken as the extreme limits of the surrounding medium compatible with life, the amount of oxygen varies from 1 to 10.

The action of the sulphide of carbon upon the vine has been examined by M. Boiteau. He finds that all doses destructive to the *Phylloxera*, from 6 or 7 grms. up to 10 grms. per hole,—*i.e.*, 12 to 24 grms. per square metre,—are injurious to the roots within a certain radius.

M. G. Pouchet states that Averrhoes is the first writer who gives an approximately true account of the sensation caused by the touch of electrical fishes. He compares it to magnetism, whilst Galen and others had considered it analogous to cold.

It is asserted by some bird-fanciers that the feathers of canaries may be turned red by feeding them, before and during the moulting season, with a mixture of hard-boiled egg and bread, dusted over with the best quality of capsicum in fine powder.

The "Revue Industrielle," after stating the conflicting views of Vegetarians and of the friends of a mixed diet, thinks that sufficient attention is not paid in this controversy to differences of race, temperament, climate, and employment.

M. Richet has investigated the effects of heat on the functions of the nerve-centres of the crayfish. Nervous voluntary action is weakened at 23° to 24°, and disappears completely at 26°. Reflex nerve action disappears at 27° to 29°, at 30° the respiratory flabellum ceases to act, and from 32° to 34° the motor nerves become inactive. It is remarked that amongst the Vertebrates

the different functions of the nervous system are affected in a progression very similar to that traced among the Crustaceans.

According to "Le Journal d'Hygiène" the heron has on its breast large greasy tufts, which secrete a whitish unctuous matter of a disgusting odour, but which has a remarkable power of attracting trout, and probably other fishes. M. Noury, on placing the breast of a heron in a net, has invariably found it filled with trout.

With regard to the chemical structure of bones, Dr. Carl Aeby points out that the hygroscopic properties and the relations of density and volume, both among the different bones of a skeleton and among the strata of one and the same bone, show greater discrepancies among themselves than is often the case with the bones of entire genera. He considers these differences as points of departure for further research, the significance of which extends far beyond the sphere of physiology.

The larva of a Dipterous insect, probably belonging to the genus *Syrphus*, has been observed to devour the *Phylloxera* with great eagerness.

M. Ch. Chamberland shows that the germs or spores of *Bacillus subtilis* in neutral liquids resist a temperature of 100° C. for several hours, and multiply rapidly at 50°.

The idea prevails in various parts of France that it is impracticable to strip the bark of trees if a flock of sheep is passing to the windward.

According to M. J. G. Lemmon the age of the *Sequoias* of California has been greatly over-rated, none of them, according to the number of their annual rings, exceeding fifteen hundred years.

Prof. Gunning declares that recent experiments have much shaken his faith in the supposed destructive action of alcohol upon low forms of organic life. There is further no *à priori* reason to be urged in favour of the assumption that alcohol is fatal to bacteria and their germs. Its dehydrating power cannot have such an effect, since such low organisms are merely thrown into a latent condition when temporarily deprived of water.

M. Coursserrant, in a communication made to the Academy of Sciences, recommends the systematic use of coloured glasses in cases of colour-blindness. In experiments which he has undertaken he finds that on looking through a green glass at a sheet of white paper, upon which was pasted a narrow stripe of cherry-red paper, the latter not merely appeared more brilliant, but a rose tint was spread over the whole white surface. According to certain experiments made upon himself, the luminous sensation is still further heightened if a feeble constant current —say of 2 or 3 Gaiffe's elements—is passed through the retina at the moment of the production of the phenomenon.

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The Report of the Inspector under the Vivisection Act for the year 1878 has made its appearance. The greatest number of licenses in force at any one time during the year was 45: of these 18 remained a dead letter. Under the remaining 27 about 481 experiments were performed, of which not more than 40 involved the infliction of serious pain. In Ireland 10 licenses have been issued, only 5 of which have been acted on, and no pain has been inflicted even in one of the 24 experiments performed. These statistics are surely enough to show the utterly farcical character of the anti-vivisection agitation which gave birth to the Act in question. Even if vivisection were a demonstrated evil, biologists might have been protected by the maxim "De minimis non curat lex."

Writing on the physical conditions of consciousness, Prof. A. Herzen says that a psychical act considered objectively is a molecular movement, *sui generis*, which an external impression conveyed from the afferent nerves to a reflex sensation induces in the central nervous elements; it is not yet psychic until the vibrations have reached a cellule of the grey substance, and it is no longer psychical from the moment when the vibrations cease or leave the central cell to communicate themselves to the afferent nerves to be expended in the form of muscular motion. The phenomenon, taken as a whole, presents two phases: in the first there is decomposition of the substance of the nervous elements, and a release of the latent energy which has been shut up; in the second there is a re-composition of their substance, and a storing up of latent energy, destined to serve for future expenditure. The author calls the first phase nervopsychic disintegration, and the second nervopsychic redintegration. He then proposes the following physical law of consciousness:—Consciousness never accompanies the integration or redintegration of the nervous elements; it accompanies merely their functional disintegration. Its intensity is simultaneously in direct proportion to the intensity of the disintegration, and in inverse proportion to the ease and the rapidity with which the internal work of each nervous element is discharged upon another sensitive or motor element, whether central or peripheric.

M. E. Heckel has presented to the Academy of Sciences an account of the action of the salts of strychnin upon the Gasteropod mollusks. Noting the fact that the alkaloids occur more frequently and possess a more powerful physiological activity the higher we ascend in the scale of vegetable life, he asks whether their special task may not be to defend the plant against animal enemies, especially as the more important an organ the more generally it is possessed of poisonous properties. He therefore finds it important to ascertain the action of the best known alkaloids upon certain selected terms in the animal series. For this purpose he has experimented with strychnin sulphate and oxalate upon *Helix pomatia* and *aspersa*, and *Zonites algirus*.

He observed that whilst *Helix aspersa*, of the mean weight of 6 to 6·70 grms., succumbed to a dose of strychnin sulphate or 0·025 grm., *Zonites algirus* of 8 grms. and *Helix pomatia* of 9·70 grms. perfectly resisted doses of 0·045 grm. He concludes that the Gasteropods enjoy a remarkable immunity from the effects of strychnin: that with them, as in case of the Vertebrata, the degree of hurtfulness of the poison is inversely as the weight of the animal, and that upon them as upon higher animals strychnin is tetanising.

Chemistry and Technology.

A writer in "Reimann's Färber Zeitung" points out that tartar emetic, as used in cotton-dyeing, serves not to fix the aniline colours themselves, but merely to fasten the tannin, thus playing the part of an indirect mordant. Several experiments have been instituted at the Färber Akademie in order to ascertain whether this application of antimony can be pronounced injurious to public health. It was found that water, in which cotton yarns dyed with aniline colours on a mordant of tannin and tartar emetic had been steeped, or especially boiled, gave distinct indications of antimony when tested in ordinary manners. The quantity of the metallic compound fixed on the fibre seems, indeed, far too trifling to have any effect upon human health. Still, in view of the panic which has seized upon the public mind, and of the tendency of the literary and political press to attribute all mysterious attacks of sickness to the influence of poisonous dyes, &c., Dr. Reimann—in our opinion very judiciously—advises dyers to dispense with the use of antimony, which will doubtless be found by no means necessary.

The effects of inhaling oil of turpentine have been described to the Academy of Sciences by M. Poincaré. He has examined 282 workmen who use this oil in their trades, and has kept animals for several months in a medium strongly charged with its vapour. Among the workmen the symptoms were headache, dizziness, watery eyes, weakness of sight, especially in artificial light, cough, and troubled digestion. In most cases the constitution became habituated to this agent, but sometimes a change of employment was necessary. The animals experimented upon remained in a normal condition if good ventilation was maintained. In confined air death ensued in consequence of congestion of the brain, lungs, &c.

Physics.

Dr. Henry Draper, the eminent American physicist who is now on a visit to this country, has addressed recent meetings of the Astronomical and Physical Societies on his alleged discovery of oxygen in the sun by bright lines in the solar spectrum. He said that hitherto he had not been able to find these lines projecting from the limb of the sun like hydrogen, and his impression

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is that oxygen resides lower than the reversing layer. He had lately been extending the dispersion of the spectrum of terrestrial oxygen, and from a light of maximum intensity of one-candle power had now got a dispersion of 80 inches from A to O. He exhibited two of the original negatives of the solar spectrum, showing the bright lines. "The Times" gives some interesting particulars respecting the time and labour involved in this research. The bobbin of the Gramme machine revolves once for each spark used in obtaining the photographs of the air spectrum. Each photograph requires 30,000 sparks, and the photographs obtained in the last three years have required no less than 20,000,000 sparks, so that the bobbin of the Gramme machine has revolved 20,000,000 times. Although the petroleum engine used for driving the machine consumes only a couple of drops of oil at each stroke, 150 gallons have been used up in three years.

At the recent Soirée of the Royal Society several novelties of great scientific interest were exhibited. In the first room were exhibited Mr. Crookes's Exhausted Tubes and Apparatus, illustrating various phenomena connected with Molecular Physics in High Vacua. A description of these experiments will be found in the "Monthly Journal of Science" for June. In another room Prof. Guthrie, F.R.S., exhibited Broken Glass in Frames, illustrating the Fracture of Colloids. The working of the Writing Telegraph was shown by Mr. E. A. Cowper. The object attained by this instrument is that it enables the operator to write at a distant station many miles away, just as though he were present there himself, without requiring the use of any special signals, codes, or signs (to spell each letter as is now the practice), and without the assistance of any person to translate the signals as received. The instrument acts upon the simple principle of communicating at all times to the writing pen at the receiving end of the line, the exact position of the pencil of the operator at the sending station through two line wires, or, so to speak, giving the latitude and longitude of the pencil continually, the position of the pencil vertically being communicated by one wire, and the position horizontally being communicated by the other wire. The pencil of the operator has two light "contact rods" jointed to it, and one of these slides over the edges of a series of "contact plates," having various resistances interposed between them and the line wire, while the other rod slides over a second set of such plates connected to the other line wire ; at the receiving end of the line each of these wires actuates its own needle. The two needles (which are placed at right angles to each other, and are provided with light springs) actuate one writing pen, so that the pen moves up or down, and backwards or forwards, in exact obedience to the motions of the pencil in the hand of the operator at the distant station. Both the paper written upon in pencil by the operator at the sending station and that written upon in ink by him at the receiving station move

along as the writing proceeds, and the messages have only to be cut off from time to time, wound round a piece of card, and sent out to their destination, or put into an envelope and dispatched. Edison's Loud-speaking Telephone was exhibited by Mr. Arnold White and Mr. C. P. Edison. This instrument far surpasses every other form of telephone at present invented. It is thus described by Mr. Conrad Cooke. A cylinder composed of precipitated chalk, to which a small proportion of acetate of mercury is added, the whole being moistened into a saturated solution of caustic potash and moulded into a cylindrical form by being subjected to hydraulic pressure, is mounted upon a horizontal axis, and is made by suitable mechanism to revolve beneath a metallic strip, which is maintained with a uniform pressure against the surface of the chalk. At the point where the strip rests upon the cylinder a small plate of platinum is fastened, and the opposite end of the strip is attached to the centre of a diaphragm of mica, four inches in diameter, firmly fixed to the framing of the instrument by its circumference. By connecting the strip to the zinc element of a voltaic battery, and the chalk cylinder to the copper pole, and rotating the cylinder at a uniform speed away from the diaphragm, it will be found that, when no current is passing, the friction between the moistened surface of the chalk and the platinum strip is sufficient to drag the centre of the diaphragm inwards, and it will take up a fixed position of equilibrium when the frictional pull in the centre of the diaphragm is equal to the elastic tension of the strained diaphragm. The moment, however, that an electric current is allowed to pass between the strip and the cylinder, electro-chemical decomposition takes place, the friction between them is reduced, and the diaphragm, finding its elastic tension unopposed, flies back to a second position of equilibrium dependent upon similar conditions; and, if a variable or undulatory current of electricity be transmitted through the instrument, the diaphragm will be kept in continual motion by the constantly varying friction existing between the chalk and the platinum dragging the diaphragm in opposition to its own constant elastic tension. So marvellously sensitive is this simple mechanical arrangement to the smallest as well as to the most rapid and complicated variations in electrical intensity taking place in the transmitted current, that all the complex sonorous undulations propagated by human articulation instantly produce their corresponding variations of frictional resistance, and the diaphragm reproduces, in a loud voice, the words which are being uttered into the telephone at the distant station. The cause of the superiority in power of this instrument over others must be looked for in the fact that the vibration of the diaphragm is produced, not as in other telephones by an electric current, but by local mechanical means, that is to say, by the rotation of the cylinder by hand, by clock-work, or by any other motive power. The function of the electric current is purely a controlling and regulating one; it determines how much or how

little of the force exerted on the spindle is to be communicated to the diaphragm, and the time when that force is to be brought into play, and it may, mechanically speaking, be compared to a frictional clutch, coupling a machine to a steam-engine, and which, at any moment, may be made to transmit the full power of the motor to the machine, or, by reducing the friction, to transmit only a portion of that power.

The Parliamentary Committee on Electric Lighting have concluded their labours and issued their report. We shall refer to the evidence in a future number of this journal. In their report the Committee state that the general nature of the electric light has been well explained in the evidence of Prof. Tyndall, Sir William Thomson, Dr. Siemens, Dr. Hopkinson, and others. It is an evolution of scientific discovery which has been in active progress during the whole of this century. Essentially the electric light is produced by the transformation of energy either through chemical or mechanical means. The energy may be derived from a natural force, as, for instance, a waterfall, or through combustion of a material in the cells of a voltaic battery, or of fuel in a furnace. The energy being converted into an electric current, may be used to manifest electric light by passing between carbon points, or by rendering incandescent solid bodies, such as iridium. A remarkable feature of the electric light is, that it produces a transformation of energy in a singularly complete manner. Thus the energy of one-horse power may be converted into gaslight, and yields a luminosity equal to 12-candle power. But the same amount of energy transformed into electric light produces 1600-candle power. It is not therefore surprising that while many practical witnesses see serious difficulties in the speedy adaptation of the electric light to useful purposes of illumination, the scientific witnesses see in this economy of force the means of great industrial development, and believe that in the future it is destined to take a leading part in public and private illumination. There is one point on which all witnesses concurred, that its use would produce little of that vitiated air which is largely formed by the products of combustion of ordinary illuminants. Scientific witnesses also considered that in the future the electric current might be extensively used to transmit power as well as light to considerable distances, so that the power applied to mechanical purposes during the day might be made available for light during the night. So far as the practical application of the electric light has already gone there seems to be no reason to doubt that it has established itself for lighthouse illumination, and is fitted to illumine large symmetrical places, such as squares, public halls, railway stations, and workshops. It is used in Paris for lighting shops which require a light by which different colours may be distinguished, and has recently been used in England for the same purpose with satisfactory results.